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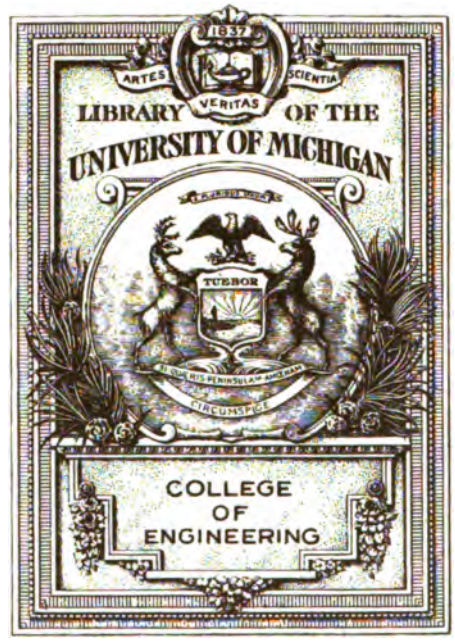
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FOREWORD.

In view of the continually increasing depths from which mining will have to be carried on in the immediate future on the Witwatersrand Gold Fields, and also on many other mining areas throughout the world, the question of winding ropes, and the conditions which they are now, and will be still more, required to meet becomes of paramount importance. The subject has been kept well in the forefront for many years, and much investigation and research have been devoted to it by South African engineers. The problems connected with the subject have been dealt with in numerous papers and discussions which have been recorded in the *Transactions of the South African Association of Engineers*, the *Journal of the Transvaal Institute of Mechanical Engineers*, and the *Proceedings of the South African Institution of Engineers*, into which the two previously-mentioned societies were merged in 1911. The Committee of the present Institution have now deemed it advisable to bring into a single book for easier reference these records which are scattered through so many volumes of the above-mentioned societies from 1902 to the present date.

The first serious consideration of the many questions surrounding the use of wire ropes was initiated by Mr. Hans. C. Behr's paper of 1902, entitled " Winding Plants for Great Depths." Leading extracts from this, and from the subsequent discussions, open the present compilation, followed by all the papers, or extracts therefrom, and discussions dealing directly with the subject of the rope itself, its manufacture, adoption to given conditions, use, life and preservation. The various contributions are given in chronological order.

As the subject has been followed up on the Witwatersrand there appears a tendency to deal with the various stress under two heads:—

(1) The consideration of the rope as regards the static stress from the point of suspension to the load, which may be considered subject to fairly uniform conditions, and more definite factors; and

(2) The consideration of all other strains to which the rope is subjected, such as the bending and torsional stresses on drum and sheave, stresses due to starting, acceleration and retardation, to kinetic shocks, etc. All of these latter stresses are more complicated, as they vary so greatly with the winding equipment and conditions of each individual installation.

Much interesting matter dealing with the incidence of the various stresses due to starting, acceleration and retardation on the safety factor will be found in Mr. Hans C. Behr's paper on " Winding Plants for Great Depths," and the subsequent discussions. The consideration of these stresses is so bound up with engine and drum construction as to constitute a very wide subject of itself.

Although in this compilation the subject of wire ropes is dealt with somewhat parochially as applied to the deep level mines of the Witwatersrand, the various questions considered are of world-wide importance, and it is hoped that the present volume will be of assistance to mining engineers engaged in confronting similar problems in many other mining districts, and will lead to wider research and collaboration in the solution of the many questions surrounding the subject of deep winding, from both the safety and economic viewpoints.

No attempt is made to strike any balance between the older and newer ideas, or to emphasize opinions which must obviously become more correct as both research and experience advance.—Ed.

PREFATORY NOTE TO MR. H. C. BEHR'S PAPER ON "WINDING PLANTS FOR GREAT DEPTHS."

It is only possible within the scope of the present compilation to give extracts which bear directly on the subject of wire ropes from the historic paper of Mr. Hans C. Behr, and the subsequent discussions. The original paper and discussions formed two bulky volumes of the *Transactions of the South African Association of Engineers*, Vol. VI, Part I, of 420 pages, containing the paper and discussions, and Part II containing 79 separate plans, diagrams and tables.

The paper dealt with the subject of deep winding mainly from the view of the winding system and winding engines employed, and the respective merits of single lift versus stage winding. Wire ropes were also considered, and the question of the use of taper ropes as against uniform section ropes was fully dealt with.

The paper was also read before the Mining and Metallurgical Engineers' Society in London, and some extracts are given from the discussion by that society. In the Johannesburg discussion the subject of the rope bulked more largely, and more copious extracts are given.

The Editor regrets that he has been compelled to be very drastic in taking out the extracts which follow, and especially regarding the subjects of acceleration and retardation, but these are so interwoven with the subjects of winding engines, drum form, and winding conditions generally that readers who are interested must perforce be referred to the original volumes mentioned above. In his final reply, Mr. Behr, in speaking of the wide field taken by the discussion, remarked that "He expected the discussion upon his paper would be chiefly directed to two subjects, viz.: (1) The comparison as to cost and efficiency between hoisting by sheaves and hoisting by conical drums; and (2) the comparison between hoisting by single lift and hoisting in stages." (Page 353, Vol. VI, Part I).

The following extracts deal only as far as possible with the rope itself, and the stresses affecting the safety factor, but give the many suggestions regarding wire ropes which largely prompted the research and investigation detailed in the succeeding specialised papers which form the bulk of the present compilation.

Page and illustration reference numbers are retained as they appeared in the original publication.—Ed.

Winding Plants for Great Depths.

By HANS C. BEHR, *Member.*

In many extensive deep-level mining operations, thanks to modern large-scale methods, the present works are being so rapidly exhausted of their ore that, in order to be able to continue the enterprise further, developments at still greater depths become necessary. A depth of nearly 5,000 ft. has already been reached in the Lake Superior copper region, and the proposition of depths exceeding this is at present being seriously considered.*

The chief and most difficult problem with increased depth is that of the winding plant. A number of different arrangements have been proposed as a solution.

Opinions seem mainly divided on the question as to whether it would be possible or economically preferable to raise the load in a single lift from, for example, a depth of 6,000 or even 8,000 ft., or whether it might not be necessary to resort to winding in stages with intermediate dumping and loading stations. The single lift for such depths necessitates taper ropes, the manufacture of which, however, presents no difficulty, while the wear ought reasonably not to exceed that of ordinary ropes under similar conditions.†

In fact a taper rope of material possessing high ultimate strength, like plough steel, and with a factor of safety of 7, is hardly warranted for a depth of 3,000 ft., at least not with cylindrical drums. Its use on conical drums admits of a lower ratio of end diameters, thereby reducing the necessary maximum diameter and the moment of inertia of the drum.

* * * *

(*Page 37*). It was shown in a preceding paragraph that the ratio of end diameters could be reduced by the use of tapering ropes. Such reduction can be made still greater if the ropes are made of material having a higher ultimate strength, and permitting, therefore, with the same factors of safety, a greater working stress for the material, so that the ropes can be made still lighter. The smaller diameter of the taper ropes also gives less width of drum, and, since the wires are also generally smaller, there will be for the same diameter of drum less stress in the wires due to bending.

There are now used in a large number of places, ropes made of material having an ultimate strength of 240,000 to 250,000 lb. per sq. in., without experiencing troublesome brittleness of the wires. With an initial factor of safety of 7, based on an ultimate strength of material = 245,000 lb., and on the static load at rest, this would give a load stress of 35,000 lb. In all preceding cases the stress was taken at 30,000 lb., making the ropes very much heavier at greater depth.‡

The influence of the factor of safety becomes more marked with increasing depth, much more so however for ropes of uniform section than for tapering ones.

---

* The Tamarack Mining Co. has three shafts, 4,450, 4,650 and 4,750 ft. deep respectively. The Calumet and Hecla has a shaft 4,900 ft. deep.

† Round taper ropes were tried at some of the mines of the Comstock as early as 1875. One of these ropes was used to operate a 37-degree incline, 1,400 ft. long, through a shaft 1,400 ft. deep, from a winder at the surface. For such conditions, the use of a taper rope would not be warranted, even if the fact be taken account of, that the material available at that time was of lower ultimate strength and much inferior in quality to that now in use. From the records of the mine, which were traced up at the writer's request by the manufacturers of the rope (A. Roebling, Sons & Co.), it appears that the rope was in use from 1875 to 1884. Probably trips were not very frequent during this time. The rope was 2 in. at the top and 1¾ in. at the lower end. The load at the end of the rope was 10 or 12 tons. Roeblings furnished four taper ropes for the mines on the Comstock. One of these worked only 9 months in the Savage Incline, and was then taken out, not on account of deterioration of the rope, but because the expense of operating the incline was too great.

At the mines of Przibram, taper ropes were used over 20 years ago for a vertical depth of about 3,900 ft. In the "Constructeur," Reuleaux gives the following data regarding them.

They were built of crucible-steel wire having an ultimate strength of about 120,000 lb., and were made with a very regular taper, starting from the small end. One wire was cut at about every 16 ft. and a larger one was brazed on. Consecutively in the process, larger ones were joined on until the rope was complete. Each rope was composed of 7 strands around a hemp centre, each strand consisting of 6 wires around a hemp core. The initial factor of safety due to static load conditions alone, and, without considering the effects of bending, was about 7, and it is stated that the ropes lasted from 3 to 4 years.

‡ In the United States, factors of safety as low as 5 are often used, based on load at rest and not taking account of stress due to bending.

Since cylindrical drums without tail-ropes should properly only be used for moderate depths, generally not exceeding say, 2,000 ft. vertical, it will not be necessary to consider them in connection with a lighter rope made of material of higher strength, the effect of the reduced weight having only a slight influence for moderate depths. With tail-ropes, which permit of using cylindrical drums for somewhat greater depths, taper ropes cannot be used; but the greater weight of rope does not affect the static conditions, only adding to the moment of inertia. In the following, therefore, the investigations will be confined to conical drums, as they seem to be the only ones suitable for great depths.

* * * *

[*In the course of a series of suggested equipments for winding from various depths the Author considers winding from a depth of 6,000 ft. vertically in a single lift, assuming a load of 10,000 lb., and a skip weighing 6,600 lb., and winding at the rate of 100·5 tons per hour. This is several times referred to in the discussion as Case IX.*—ED.]

(*Page 41*). The rope must necessarily be tapering, or rather made of sections increasing in size towards the top. The lowest section of a tapering rope is properly made a little longer than the others, so as to give a somewhat higher factor of safety at the lowest point, where attachment is made to the skip, and where breaks most frequently occur. In the present case, the rope is made in four sections, the lowest and longest being 1,800 ft., while the other three are 1,400 ft. each. The proportions and weights of the sections of rope are shown as under:—

Top section, 1,394 ft., weight 4,200 lb., diam. of rope 1·60 in.
Next section, 1,400 ft., weight 4,740 lb., diam. of rope 1·45 in.
Next section, 1,400 ft., weight 3,980 lb., diam. of rope 1·33 in.
Lower section, 1,800 ft., weight 4,300 lb., diam. of rope 1·22 in.

The total weight of the rope is 18,676 lb., based on an ultimate strength of material of 245,000 lb., and a load factor of safety of 7 at the upper ends of the sections. For the same factor and strength of material, a rope of uniform section would weigh about 38,000 lb., or over twice as much as the taper rope. The diameter of the rope of uniform section will be about 2 in., while the average diameter of the taper rope will be about 1·4 in., so that a drum about 40 % wider per coil of rope would be required for the rope of uniform section. The drum shell would also have to be thicker on account of the greater strain due to the extra weight of rope. The weight and moment of inertia of the drum would therefore be increased in proportion to the greater width and thickness of shell, if the other dimensions of the drum remain the same in both cases.

But if equal static moments are desired at the ends of the trip for both cases, then, if the small end be 10 ft. diam. for both drums, the larger diameter in the case of the rope of uniform section would have to be over 40 ft., while for the taper rope as above it will only have to be 26 ft. The larger diameter, though reducing the width of drum, would give an enormous increase of weight and moment of inertia, while the very steep cone would aggravate the danger of the rope slipping in winding, which already is serious with the slope of the drum of 26 ft. diam. It will therefore only be of interest to investigate the latter.

In connection with deep winding, it may be noted, in parenthesis, that with great depth it will probably be found, nearly always, most economical to use a rather low factor of safety and change the ropes more frequently, than to employ a high factor of safety with a longer period of use for the ropes. Such a proceeding would give more favourable drum proportions and smaller size of engine or motor, with corresponding reduction in first cost. The saving in interest and depreciation, and in the amount of power required, would generally be greater than the extra operating cost due to more frequent replacement of ropes.

Where separate winders are used for men and rock, as will probably be necessary in all deep-winding propositions, the ropes could be first used on the man-hoists with a suitable factor of safety, and, after a certain amount of usage, be transferred to the rock-hoist, in which a much lower factor of safety is generally admissible, so that quite a long period of use can be had for the ropes in such cases.

* * * *

(*Page 52*). It is of course evident that circumstances must decide which system of stage-winding is the proper one to choose in each case, or whether a single lift might be the best plan to adopt. The foregoing

analyses and discussions have shown the more important defects of the single lift when applied to such vertical depths as 6,000 ft. It is probable, however, that the single lift may in many cases be best for a vertical depth of 4,000 or even 4,500 ft. For inclines of moderate angle, the ratio of end diameter of the cone drums is not so great, and more favourable conditions then exist for the employment of the single lift, even for inclines of greater length.

* * * *

LONDON DISCUSSION.

[*The following are extracts from the discussion of the paper by the Institution of Mining and Metallurgy in London.*—ED.]

(*Page 63*). Mr. L. F. WHITMORE said Mr. Behr inferred that the Whiting system was not a suitable one for great depths. He (the speaker) was half inclined to think that there was a difference of opinion on this point. Calculating it out roughly, taking the ultimate stress of the ropes, as given on p. 38, and the same load, 6,000 lb., of ore with a skip of 4,000 lb., a Whiting hoist with a factor of 7 to 1 with 1¼ in. diam. rope would hoist from a vertical depth of probably 5,000 ft., with 1½ in. rope it would hoist from a depth of about 6,000 ft., and with a 1¾ in. rope from a depth of 7,000 ft. At these great depths, in order to get the necessary amount of ore to the surface per hour, the load would probably have to be increased to make up for the extra time taken to do the wind, in which case with 1¾ in. diam. rope about 8,500 lb. could be hauled from 6,000 ft. If they adopted the lower factors, which, it was stated, were adopted in the United States, of 5 to 1, he believed it would be possible to wind from a depth of nearly 10,000 ft. by 1¾ in. diam. rope. These figures would apparently bear out the opinion that the Whiting system was very adaptable for great depths. The Whiting system was certainly in use at 5,000 ft. at the Calumet, and there were many hoists built for that depth and greater. As a matter af fact there were seven hoists being built and already built for the Rand, to haul from a depth of 5,000 ft. and probably exceeding that.

* * * *

(*Page 77.*) THE PRESIDENT (Mr. A. G. CHARLETON) said Mr. Whitmore had made the remark that the factor of safety in the United States was not as high as that which appeared to be required in the Rand, one being 7, and the other 5; and this was another matter that must be taken into account, as it would be apt to modify the conditions which had to be provided for; but doubtless a high factor of safety was desirable, especially when winding from great depths.

* * * *

(*Page 80.*) Mr. BENNETT H. BROUGH: Owing to recent progress in steel manufacture, it is possible to have cast steel wire-ropes with a tensile strength of over 114 tons per square inch. In other words a rope of such material may be 59,000ft. long before it broke from its own weight. It seems, therefore, that with winding in two stages there was ample scope for the extended use in the future of the Lake Superior practice of using ropes of uniform section. The results obtained with taper ropes of special cast-steel wire at Przibram, in Bohemia, have been dscribed in great detail by Mr. Carl Habermann.* The results recorded, especially with ropes of English manufacture, are certainly admirable. Great difficulty was, however, experienced owing to the liability of corrosion shown by all ropes of that kind wherever manufactured.

* * * *

(*Page 81.*) PROFESSOR C. LE NEVE FOSTER: I find in notes I made at the Paris Exhibition, 1900, that the "Chatillon, Commentry and Neuves-Maisons" Company exhibited a specimen of wire rope made of nickel steel with a tensile strength of 210k. per square mm., or say 135 tons per square in. In addition to its great tensile strength, this metal has the further advantage of not oxidising. It may possibly have a great future in the manufacture of wire ropes. At present its price is so high that its use would not be allowable save in special cases.

* * * *

(*Page 91.*) Mr. BEHR, replying to the discussion, made the following remarks on the subject of Wire Ropes:—Speaking of taper ropes Mr. Whitmore claims that they are less secure at the lower end than a rope of uniform section, ropes generally breaking at the end where they are attached to the cage. This greater strength of ropes of uniform section at the point of attachment to the load is only secured where no tail-rope is used. The lower part of taper ropes

* "Oesterreichische Zeitschrift," vol. xliii, p. 193

is properly made of uniform section for a considerable length, so that the factor of safety is greater at the end where attached to the load than at the drum. Thus if the factor is 7 at the upper end of the part of uniform section, it will be over 9 at the end attached to the skip if the part of uniform section is about 2,000 feet long.

* * * *

(*Page 94.*) Some of the contributors to the discussion expressed a hopeful view of the improvement in rope material in the near future, so as to meet the necessities of deep winding as soon as they developed. Considerably stronger material than the best special so-called plough steels now used has been made and used in ropes; but manufacturers rightly do not recommend such material for winding ropes, on account of its brittle nature and its liability to give way suddenly under shock. Piano wire has a tensile strength 25% to 50% higher than that of the plough steel referred to; but it can only attain this strength by being drawn to a smaller size than is advisable for winding ropes, for the wires of these ropes should be stout enough to prevent corrosion and wear reducing their cross section too rapidly for economy in working. It seems posible or even probable, however, that improvements in rope material will be made; but how soon? It would seem wiser to rely on known material than to calculate on what the future might have in store, and to design plants suitable for the materials of to-day.

* * * *

(*Page 95.*) I fail to reconcile Mr. Brough's statement, that all taper ropes give trouble from corrosion, with the fact that taper ropes are being used at Przibram, and are said to last often three years.*

JOHANNESBURG DISCUSSION.

The following are extracts from the discussions on the paper by the South African Association of Engineers at Johannesburg.—EDITOR.

(*Page 101.*) Mr. E. T. SEDERHOLM contributed the following remarks on the subject: The Rand Mines, Ltd., had adopted as a standard an engine to lift four short tons of rock from a maximum depth of 5,500 feet. This required a plough steel rope of only 1½ in. diam. on drums 12ft. in diam., and such a rope over 12ft. drums was strained considerably less per square inch of net section than the 1.6in. taper rope which Mr. Behr proposed to use on a drum 10ft. in diam. at the small end. He would refer to this later on, but mentioned it here to show that, contrary to Mr. Behr's idea, there was no difficulty at all in using a rope of uniform section for depths as great as 5,500ft., and even greater if necessary.

* * * *

(*Page 103.*) In speaking about safety of ropes, Mr. Behr assumed that a factor of safety of 7 should be used, although he said that in the United States a factor as low as 5 was common. This was correct, but not in the sense employed by Mr. Behr. He calculated the strength of the rope as if it were a straight wire loaded at one end and held fast at the other. In reality, the wires composing the rope were subjected not only to the strain due to the direct pull of the load, but also to the strain of bending around the drum, which was often of small diameter. This bending strain was very serious, and could not safely be neglected. It was extremely difficult to calculate this bending strain exactly, on account of the complicated shape of each wire as it was laid in the rope, and of the friction between the different wires, which could not well be estimated. It would, however, be seen that these two factors operated in opposite directions. Each wire, instead of being simply a straight wire bent around the drum, formed a spiral, and this spiral as a whole formed another spiral. The result was that instead of a plain bending strain being set up, the strain became a combination of a bending strain with a torsional strain, and, as a mater of fact, this torsion or twist was such that it lessened the strain which would arise if it were attempted to simply bend a straight wire around the drum. On the other hand, as stated above, the wires were held together by friction, so that the rope forms a fairly solid mass. Each wire could therefore not adjust itself perfectly to the strain, and as a consequence, when the rope was bent around the drum, the wires were more strained than they otherwise would be. The condition might, therefore, be assumed to be the same as if we had a loose bundle of straight wires and attempted to bend them around the drum. While this was not strictly correct, it was probably as near the

* C. Habermann "Oest. Zeit. für Berg-und Hüttenwesen," 1890, p. 430; 1895, p. 193.

truth as we could get, and certainly ropes calculated on such an assumption bore out this view, since they agreed very closely in size with the ropes used in best practice. In calculating the size of ropes, he had therefore always separated the two strains, that due to the load alone, and that due to the bending alone. Adding the two together he obtained the total working strain in the rope. The accompanying graphical table (Plate XXVI), worked out by him some years ago, would further illustrate what he meant. From this it would be seen what an important *rôle* the diameter of the drum played in the strength of ropes, and he would like to call Mr. Behr's attention to the fact, that, in his different assumptions of strength of rope, the actual strain varied considerably, on account of this influence of the drum.

It was undeniable that a taper rope would weigh less than an ordinary rope of even section throughout; but, at the same time, it must be borne in mind that the large diameter of the taper rope was but little less than the diameter of a rope of uniform section, and since this large diameter had to be bent around the small end of a taper drum, the other end of the same drum becomes very large if reasonable balance is obtained. A perfect balance for all conditions of hoisting could not be obtained at all. When there was added to this, the comparative difficulty of obtaining taper ropes, also the fact that they have to be specially made for each depth of mine, and that, after all, straight ropes could be had that would hoist a reasonable load from even 5,500ft. depth, it appeared that the adoption of taper ropes meant going a long distance out of the way in order to obtain a small advantage, which was coupled with serious disadvantages.

(*Page 107.*) Mr. BEHR asked to be permitted to make a few remarks before the discussion proceeded.

He said that he noticed several places in Mr. Sederholm's communication where the latter had misquoted him, or misunderstood the text of his paper. This might be due to careless composition on his part; but while he did not wish to reply at full length to Mr. Sederholm's remarks at present, he thought attention ought to be called to these misunderstandings before the discussion proceeded further. This would be only fair to the original paper, and would save time in the end.

Mr. Sederholm had said:—"In speaking about safety of ropes, Mr. Behr assumed that a factor of safety of 7 should be used, although he said that in the United States a factor as low as 5 was common. This was correct, but not in the sense employed by Mr. Behr. He calculated the strength of the rope as if it were a straight wire loaded at one end and held fast at the other. In reality, the wires composing the rope were subjected not only to the strain due to the direct pull of the load, but also to the the strain of bending around the drum, which was often of small diameter. This bending strain was very serious, and could not safely be neglected."

And Mr. Sederholm had wound up by saying:—

> "From this it would be seen what an important *rôle* the diameter of the drum played in the strength of ropes, and he would like to call Mr. Behr's attention to the fact, that, in his different assumptions of strength of rope the actual strain varied considerably, on account of this influence of the drum."

He (Mr. Behr) would say here that ropes were generally rated by manufacturers, and also by legislation, on the static load, without taking account of bending stress, and that factors of safety lower than 5 had been used in this way in the United States. The Transvaal law stated that ropes, when used for winding men, should never be loaded to more than $\frac{1}{5}$th of their breaking strength. This was, he believed, the same as the Prussian State Law. In all of these cases the load was considered to be only that due to the weight attached plus that of the rope. It was left to the engineer to decide in how far he might increase the stress, not only by bending, but also by acceleration, and still retain reasonable safety.

At the mines of Przibram, in Bohemia, they used, or at least did use for a time, a simple load factor of 7 in proportioning their ropes. The curvature of the wires over the drum reduced this factor if the wires were assumed to be simply bent round the drum without being spiralled to about 3.8; what the increase of stress due to acceleration became, depended not only upon this, but also upon the elasticity and length of rope.

* * * *

(*Page 112.*) Mr. G. H. THURSTON said that he desired to make a few remarks on Mr. Behr's paper, from which the follow-

E. T. SEDERHOLM. *Plate XXVI.*

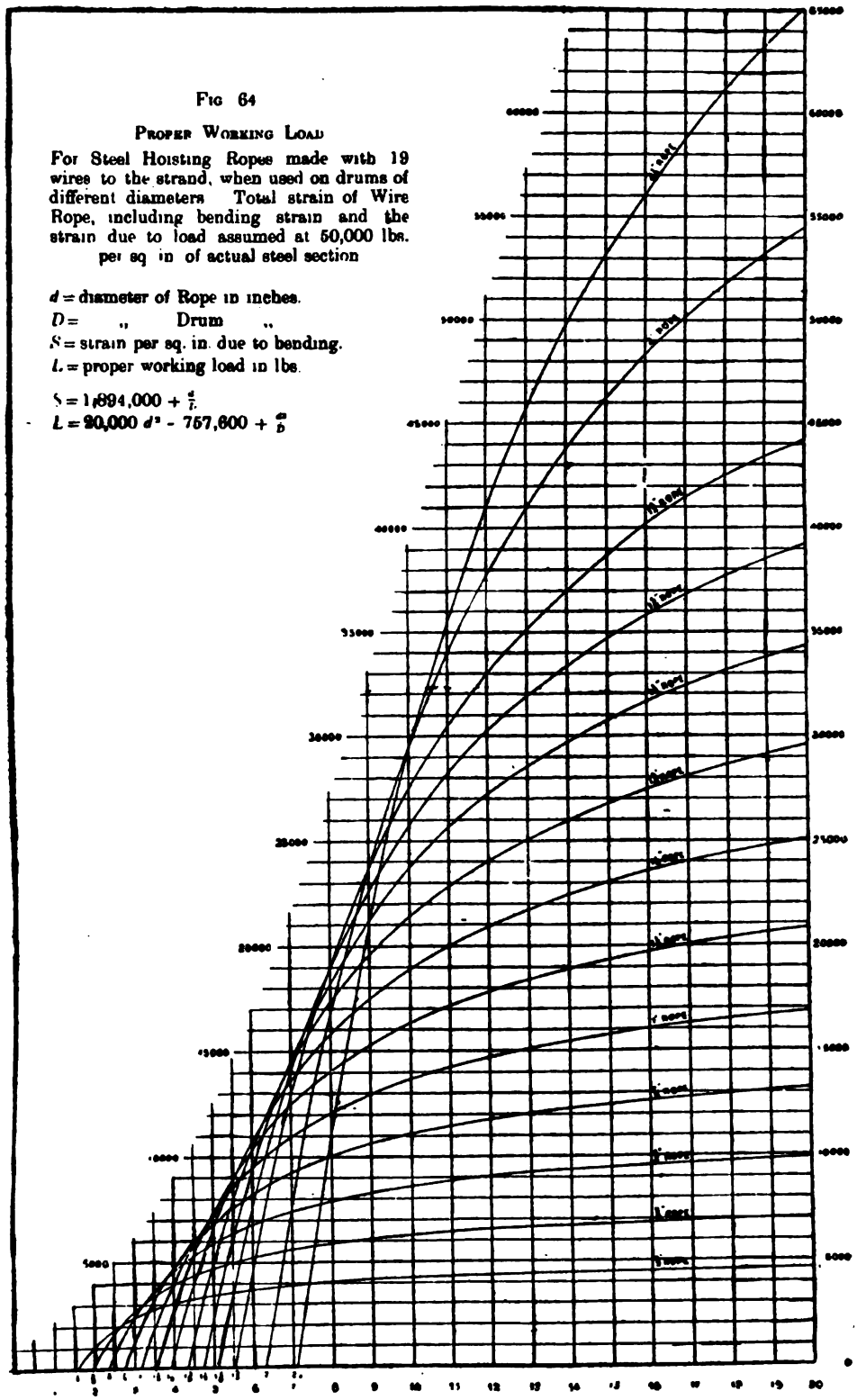


FIG 64

PROPER WORKING LOAD

For Steel Hoisting Ropes made with 19 wires to the strand, when used on drums of different diameters Total strain of Wire Rope, including bending strain and the strain due to load assumed at 50,000 lbs. per sq in of actual steel section

d = diameter of Rope in inches.
D = " Drum "
S = strain per sq. in. due to bending.
L = proper working load in lbs.

$S = 1,894,000 + \frac{d}{D}$

$L = 20,000\ d^2 - 757,600 + \frac{d^2}{D}$

ing extract is drawn:—Turning to the rope Mr. Sederholm told them that the Rand Mines were to use a 1½in. rope to hoist from 5,500ft., taking 7 as a factor of safety. With this they would then have a load of 6,000lb. weight and a skip of 4,000lb., and the weight of the rope would be approximately 20,105lb., about double the weight of the load of ore and skip.

Comparing this with a taper rope they had the following figures:—

	Length. Ft.	Diameter. In.	Circumference. In.
1st or bottom Section ...	2,000	·994	3·122
2nd Section ...	1,750	1·114	3·499
3rd Section ...	1,750	1·233	3·873

The total weight would be, approx. 11,200lb., which was only about one-half the weight of the rope of a uniform section.

Again, taking another example which had been quoted, *i.e.*, 8,500lb. of ore to be hauled from 6,000ft. with a 1¾ in. rope (5.49in. in circumference), using 7 as a factor of safety.

1st. For the 1¾in. rope they had a weight of 28,000lb. approximately, or more than double the weight of the ore and skip (allowing 5,000 lb. as the weight of the skip).

2nd. In the case of the taper rope of the following construction:—

	Length. Ft.	Diameter. In.	Circumference. In.
1st or bottom Section ...	2,000	1·174	3·688
2nd Section ...	1,500	1·273	3·999
3rd Section ...	1,500	1·392	4·373
4th Section ...	1,000	1·532	4·812

The total weight was about 17,000lb., being a difference in favour of the taper rope of 11,000lb., thus showing, that what they saved by a reduction in weight in the drums, was partly lost in extra weight of ropes.

The above calculations were based on a breaking strain of 123 tons of 2,000 lb. each per square inch.

(*Page 131.*) Mr. Hennen Jennings, in the course of his remarks, dealt as under with the subject of Wire Ropes:—

The ropes for the men's winder should be the same size as those for rock, and the new ropes should always be used first on the men's engine.

The next problem was to determine to what depth it would be feasible to use a rope of uniform section. Rope-makers were now making ropes which they guaranteed to have a breaking strength of 127 tons per sq. in. It was possible, by the employment of nickel steel, to obtain a still greater strength, but this had not yet been practically demonstrated. To be on the safe side a breaking strength of 120 tons per sq. in. would be assumed, and with this basis the following tables (2 and 3) had been calculated by Mr. Smart (rope 6 strands 19 wires and main core of hemp):—

Table 2.

Length, in ft., of rope, the weight of which (under static conditions and with no bend in rope) will equal the greatest permissible stress with different factors of safety:—

Factor of Safety.	Length of Rope in ft.
10	5,700·24
9	6,333·6
8	7,125·3
7	8,143·2
6	9,500·4
5	11,400·48
4	14,250·6

Table 3.

Diameters of rope required for hoisting a net load of 8,000 lb. and a skip weighing 5,000 lb., with factors of safety 4, 5, 6 and 7, from depths of 3,000 to 8,000 ft.

	Factor of Safety.			
	4	5	6	7
Depths. Ft.	Diameter of Ropes in inches.			
3,000	·8498	·98347	1·1180	1·2569
4,000	·8903	1·0478	1·2154	1·4004
5,000	·9372	1·1267	1·3437	1·6077
6,000	·9923	1·2266	1·5326	1·9471
7,000	1·0585	1·3588	1·8026	2·6660
8,000	1·1401	1·5458	2·3270	7·5327

TABLE 4.

Net loads of rock which can be hoisted from depths of 3,000 to 8,000 ft., with factors of safety varying between 4 and 7, with a rope 1·3485 in. diam., the weight of skip or cage being taken at five-eights of the net load.

Depths. Ft.	Factor of Safety.			
	4	5	6	7
	Net Load in Pounds.			
3,000	19,994	14,929	11,553	9,140·6
4,000	18,218	13,154	9,775·5	7,363·4
5,000	16,440	11,375	7,998·2	5,586·2
6,000	14,663	9,597·8	6,221	3,809
7,000	12,886	7,821	4,443·8	2,031·7
8,000	11,109	6,043·4	2,666·5	254·48

The conclusions arrived at in these tables were important.

Table 2 showed the absurdity of requiring too high a factor of safety, for, with a factor of safety of 10, ropes or strands of uniform section of any diameter would be considered unsafe if suspended beyond 5,700ft. It also showed the limits to which winding was possible with factors between 10 and 4, and with minimum loads.

Table 3 showed that, if a factor of safety of 6 were used, a rope of 1.3437in. diam. could safely be used to raise a load of 4 ton from a depth of 5,000 ft., and a rope of 1.5236in. diam. from a depth of 6,000ft.

Table 4 showed that, with a load of men of 4,000lb. in a cage of 2,500lb. in weight, men could be hoisted from a depth of 5,500 feet with a rope of 1.3485in. diam. and factor of safety of 7. The cage, however, weighed 4,600lb., and they would therefore take as their standard a rope of 1.5in. diam., this would give a factor of safety of over 7, for hoisting of men, and of over 6 for rock; reckoning the weight of the men to be 2 tons and that of the ore 4 tons, hoisted from a depth of 5,500ft.

Mr. Behr gave instances in his paper of only two places where taper ropes had been extensively used: one at Przibram, Austria, and then only for a depth of 3,900ft.; and the other at the Comstock, where he admitted they were not warranted, as the depth was only 1,400ft. vertical, and 1,400ft. on an incline of 37deg. He (Mr. Jennings) had no knowledge of their adoption in any other districts, and the ropes in the Lake Superior district were all of uniform section.

The following extract was taken from Kent's *Mechanical Engineer's Pocket Book* (5th edition, p. 916):—

"The true form of rope is not a taper, but follows a logarithmic curve, the girth rapidly increasing towards the upper end."

The method of construction of the ropes at Przibram was complicated, and must be costly, and Mr. Behr was no doubt wise in his suggestion to use successive lengths of rope of uniform section for hoists with conical drums. This being so, would not the place of junction of two ropes of uniform section be a "weak link in the chain," and would it not be logical to insist on a higher factor of safety in the taper rope system than in that of ropes of uniform section? At present, did the Mining Regulations permit ropes of uniform section to be spliced, and, if not, how could Mr. Behr legally join a length of rope of small section on to another length of larger section? It would also be interesting to see how the sections recommended by Mr. Behr corresponded with those demanded by the theory mentioned above by Kent.

As far as the records of the exact places where ruptures of ropes had occurred, mining accidents for moderate depths would seem to show that the ropes should taper upwards rather than downwards. In support of this seeming paradox, he desired to call attention to the following summary of a pamphlet by Hermann Undeutsch,* Professor at the Royal Saxon Mining College, Freiberg.

Professor Undeutsch prefaced his work by pointing out that the Royal Prussian Board of Mines, as well as the Chief Overseer of Mines of the Dortmund District, and the Saxon Board of Mines at Freiberg, had for many years kept statistics concerning the breaking of hoisting ropes, and that these statistics went to show that most of the ropes broke at the place where, statically, they were the strongest, that was near the load; the weakest section being that between the shaft collar and the headgear sheave. In other words the ruptures were most frequent in a part of the rope seldom wound round the sheave, where the shackles and connections are more often renewed than the rope, and where there was a minimum amount of bending stress.

By a most elaborate mathematical analysis of the static and dynamic strains on sus-

* *Spannungen aufgehängter prismatischer Körper, hervorgerufen durch statische und dynamische Beanspruchungen*, 1892, *p.* 51, with plates.

pended prismatic bodies, in connection with experiments he had made of the measurements of dynamic effects, by means of an energy indicator he had invented and used for making tests while lowering, stopping and starting cages, he arrived theoretically at the same results as those which the statistics chronicled. He concluded his treatise by stating that the elastic limit of the rope was exceeded, and the material made brittle, through vibrations set up in the axial direction of the rope; and he further pointed out that the changes occur with more intensity at the bottom, and in a lesser degree at the upper portions, and only as a secondary result.

* * * *

(*Page 139.*) The safety of life, in winding, depended primarily on the strength of the rope and on the maintenance of its condition.

The breaking of a rope was brought about, as shown by Professor Undeutsch, by the joint action of static and dynamic strains. The static strains could be taken as constant for any winding system with established factors of safety. The dynamic strains might occur by irregular and jerky winding, sudden starting and stopping, and by the imperfect condition of the hoisting ways when the engine was running at its maximum speed. The smaller the drums and cylinders of a winding engine, and the more uniform the speed, other things being equal, the less liability there was of excessive shocks from imperfect handling by engine attendants. Where an average speed of say 2,500ft. per minute was required, the maximum speed had to be much higher in the system demanding the greatest time for acceleration and retardation.

* * * *

(*Page 151.*) The most interesting and insistent recommendation of Mr. Behr was the necessity he claimed for the use of taper ropes. The theoretical advantages of taper ropes in hoisting were fascinating, especially in combination with conical drums; but, before they could be accepted, there were some very practical facts to be faced. In the first place, taper ropes had not been used in hoisting from depths which would crucially test their efficiency. In the whole history of hoisting plants Mr. Behr could give but two instances of their use, one at the Comstock, the other at Przibram. He admitted that they were unnecessary in the first case named, and the depth at which the taper ropes were used at Przibram was only 3,900ft. The smallest diameter of rope used, even at depths of 1,000ft., and with live load of 2½ to 3 tons, was 1⅛in., and the reason for this was explained theoretically by Professor Undeutsch, and practically confirmed by all mining statistics. With huge drums and consequently big engines, the shocks and vibrations are intensified, so that to safely withstand the latter, a rope should, to commence with, not be less than, say, 1¼in. in diameter for a load of four tons. Granting this, and referring to Table 3 (p. 19), it would be seen that such a rope, even if made of uniform section, would be strong enough statically to hoist a live load of four tons and a skip of 5,000lb. from a depth of 4,000ft. with a safety factor of 6. Now at such a depth as this what were the advantages of taper ropes? Ropes of uniform section had been used to 5,000ft., whereas taper ropes had never been used beyond a depth of 4,000ft.

(*Page 162.*) Mr. A. M. Robeson dealt in his discussion with many matters connected with deep winding, and incidentally referred to Wire Ropes as under:—

It is now possible to go at once to the limit, viz., 6,000ft., and it might as well be acknowledged at once, that if a safety factor of 7, calculated on the static load, were insisted on, it was useless to consider the question of operating at that depth in one lift, but if "7" were adhered to for men, and "6" for ore and materials, then it was possible.

This statement was meant to include taper ropes, because it was manifestly unfair to ask for a safety factor of 7 for so perfect a structure as a rope of uniform section, and use the same safety factor for a rope made according to the method described by the author in the footnote on page 1; as with the acid water of the Witwatersrand mines, the electric couple formed at each brazed point would soon lower the factor below 7, and if wires were "dropped" at intervals, as some makers proposed, the rope would be no better than a spliced rope.

(*Page 167.*) In passing on to winding ropes, the liability to break was not always at the shackle or capping, but breaks had occurred in the past at a point on the rope which generally reposed for a time between the enginehouse side or end of the drum.

It appeared to be the heat from the engine and the cold air from outside that had a bad effect on the rope and caused internal corrosion.

(*Page 172.*) Mr. A. C. WHITTOME, in the course of his discussion, remarked:—

"Ropes of uniform section had been used to 5,000ft., whereas taper ropes had never been used beyond a depth of 4,000ft." A statement like this showed a considerable amount of prejudice, and neutralized a great deal of the value of the remarks that he made on the subject of ropes, and, taken in conjunction with the following extract from the same paragraph:—"In the whole history of hoisting plants Mr. Behr could give but two instances of their use (tapered ropes), one at the Comstock, the other at Przibram," indicates a desire to quibble over details which were known to most engineers. He thought that there were not many shafts of a depth of over 4,000 feet in the whole world, possibly there were not more than ten, and when they came to shafts of 5,000 feet and over in depth, the number would certainly be reduced to one, if Mr. Jennings was correct in saying that the deepest shaft in the world was only down to 4,900 feet. It was on record that for the past 30 years tapered ropes had been used with very great success on shafts which 30 years ago were 3,900 feet deep, and were now slightly over 4,000 feet deep. Could Mr. Jennings instance one case where ropes of uniform section had, for such a period and under such duty, done better service than tapered ropes? Regarding the isolated instance given of a Whiting hoist hauling from a depth of 4,900 feet, Mr. Robeson said:—"Broadly speaking, the system had been a failure there, because it was not well carried out in its greatest example, viz., at the 'Red Jacket' shaft."

Surely when this one solitary trial had ended in a failure they could be permitted to reply to Mr. Jennings that tapered ropes have not been used for depths of 5,000ft.; neither have ropes of uniform section, with success.

(*Page 183*). Mr. DAVID GILMOUR mainly dealt with other branches of the subject of winding, but made the following observations on the Factor of Safety:—

He held the same opinion as Mr. Robeson, that the factor of safety for ropes, at any rate for winding ore, would be quite high enough if fixed at six times the static load, and it was with the object of insisting upon the effect that any considerable addition to this figure would have, in prohibiting the use of ropes of uniform section for great depths, that he made the following comparison for the Cases IX previously referred to.

In Case IX a factor of safety of 7 for static conditions was assumed. The increased stress due to starting was found from

$$\frac{mr}{gl} = \frac{153{,}420 \times 5}{32 \times 940{,}000} = 0255 \text{ or } 2{\cdot}55\% \text{ inerse.}$$

or, in other words, the factor of 7 was reduced to 6.8.

Any system employing ropes of uniform section, which had to work under conditions imposing such a high factor of safety for dynamic loading, even if made of wire with an ultimate tensile strength of 245,000lb. per square inch, would evidently reach the limit of depth very soon, and thus, in a measure, stifle progress in opening up the deeper levels. Therefore he repeated that the universally accepted system of fixing the factor of safety from the static load only should be adhered to for these greater depths. Owing to the total moment of inertia of the drums in the Whiting system being so small, the increased pull on the rope at starting was greater than in systems in which the drums were more massive, and in Whiting C. IX this increase was approximately 10½%. With steel wire having an ultimate strength of 245,000lb. per sq. in., and taking as the factor for safety for dynamic loading the same figure as that which resulted from Mr. Behr's Case IX above, viz., 6.8, and assuming the acceleration to be the same for the lesser depth which must result, as for 6,000ft., the maximum stress for dynamic loading would be

$$\frac{245{,}000}{6.8} = 36{,}000\text{lb. per sq. in.}$$

and for static loading it would be

$$\frac{36{,}000}{100+10.5} \times 100 = \frac{36{,}000}{110.5} \times 100 = 32{,}500 \text{ lb. per sq. in.}$$

and the static factor of safety would be

$$\frac{245{,}000}{32{,}500} = 7{\cdot}55$$

In other words, the static factor of safety must be 7·55 in order that the dynamic factor of safety may not fall below 6·8 at starting. Therefore the maximum depth x to which the specified rope (which weighs 3·6 lb. per foot) can be used to fulfil the assumed conditions would be given by the formula—

$$\frac{\text{Load in lb.} + \text{weight of skip in lb.} + \text{weight of rope in lb.}}{\text{Sectional area of steel in rope} \times \text{static factor of safety}} = \text{ultimate strength of the rope.}$$

Substituting figures for the case under discussion,

$$\frac{8{,}000 + 4{,}000 + 3{\cdot}6\ x}{{\cdot}859} \times 7{\cdot}55 = 245{,}000.$$

From this $x = 4{,}400$ ft. approximately.

* * * *

(*Page 185*). The whole question of winding plants for great depths was full of intricacies, but Mr. Behr's beautiful and simple analysis of the mathematics of the subject robbed it of half its terrors. The safety and realiability of the machines, and the devices connected therewith for accident prevention, did not depend on such formulated solutions, and the personal factors which then entered into consideration necessitated more than ordinary judgment, care, and common sense being brought to bear on the points. As a depth of about 4,500 ft. from the surface to the intersection with the reef was the greatest that was likely to be dealt with on the Rand for some years to come, it was to be hoped that, as that depth could be attained in one lift, engineers would not allow the question of hoisting from extreme depths to drop at the end of this discussion, but would continue in their efforts to find the best solution of the problems brought to a head by Mr. Behr, to whom the thanks of all of them were due.

(*Page 192*). Mr. H. Hay (*extract*): With regard to the liability of the brazed joints of the wires of taper ropes becoming the seats of galvanic action, he was informed that, in the ordinary system of rope making, the wires were drawn in lengths of from 2,000 to 3,000 ft., which were brazed together and woven into ropes of from 5,000 to 6,000 ft.; consequently this electric action might take place in any ordinary rope of uniform section.

(*Page 199*). Mr. J. A. Frerichs (*extract*): A rope of uniform section for a 5,500 ft. lift, which is the largest sized engine of Mr. Sederholm's standard Whiting hoist, would be 1·65 in. in diameter, and weigh no less than 22,660 lb.; whereas a rope, tapering in sections of 1,000 ft., the first length being 1,500 ft., for the same depth, would have diameters as follows:— 1·14, 1·22, 1·29, and 1·37 in., and weigh only 12,870 lb. These diameters were calculated as in Mr. Behr's paper for a safe load stress of 35,000 lb. per square in., the load being 4 short tons, or 8,000 lb. and the weight of the skip being 5,400 lb.

(*Page 199*). Mr. A. R. Sawyer (*extract*): During his many years' experience as one of H.M. Inspectors of Mines he had found that ropes broke mostly at the cap end. The reason was obvious; the portion of the rope enclosed by the cap was rigid, any slackening of the rope at top or bottom caused the rope to bend at the cap end and weakened it at that point.

* * * *

Mr. Kenneth Austin, *after dealing with the various winding systems, winding engines, etc., proceeded to the discussion of numerous questions relating to wire ropes, as under*:—

(*Page 247*). It was generally assumed that for the high class wire needed for hoisting ropes, only crucible cast steel could be used. The casting from the crucible gave a "billet" weighing 90 lb. after it had been thoroughly forged and finished. This weight would seem to limit the length of the wires, if it were specified that the whole 6,000 ft. or more should be drawn in one piece; but there was no reason why the makers of steel billets should not adopt the weights given in Table 14, when they knew

Table 14.

Weight of Steel Wires most generally used for Hoisting Ropes.

Imperial Wire Gauge.	Diameter.		Sectional Area.	Weight per mile.	Weight per 6,500 ft.
	Ins.	Mm.	Inches.	Pounds.	Pounds.
9	·144	3·55	·0163	282	347
10	·128	3·22	·0129	223	274
11	·116	2·93	·0106	183	225
12	·104	2·66	·0085	148	182
13	·092	2·33	·0066	114	140
14	·080	2·06	·0050	88	108
15	·072	1·86	·0041	70	86

there was a constant demand for long wires.

He wished to point out that it was very illogical to braze or connect two ends of wires in a rope, each of which had a tenacity of 120 tons per sq. in. section, by means of brass solder, which had a tenacity of only 13 to 15 tons per sq. in. section, under the best conditions.

Electric welding had been tried, but it was a complete failure when used on high carbon steel wires. It might be urged that all steel ropes had brazed wires in them, which was now actually the case, and that therefore as these had not generally failed, the practice should be continued, or even might be developed, as was proposed by advocates of taper ropes. It must be pointed out that by multiplying the brazed joints, there would assuredly be an increase in the number of the brazed joints on the outside of the wire rope strands, which would doubtless lead to trouble in use.

He submitted that the safety of deep level operations demanded that there should be continuity of connection between power and load, and that the section of any wire in a rope should be continuous, and not interrupted by a weak brass joint, having the low tensile strength of, say, 15 tons per sq. in. of section. Mr. Behr had to explain how he proposed to join the various different sections of his taper rope so as to present a regular even structure, with absence of bulges, lumps or irregularities. Some makers had used centre core wires of triangular shape, but these were dangerous, as they cut into or broke up the covering wires in the strand. For all future operations, the best construction of wire rope would seem to be that made up of six strands and fibre centre, each strand composed of 19 steel wires of regular diameter. He had noted the amount of wear, and taken account of the reduced section of wires in a number of ropes which had come under his notice, and had found in ropes 1 in. to 1¼ in. diameter, formed of six strands with seven wires in each strand, that only two of the outside wires in each strand were usually affected by abrasion, *i.e.*, were actually worn away at any section cut squarely across the rope, and in the average of cases only 20 to 50 per cent. of the original areas of these two wires in each strand were thus reduced, before the ropes were abandoned. This statement referred to ropes which had not been subjected to the corrosive effects of acid water, and where no other internal defects were observed.

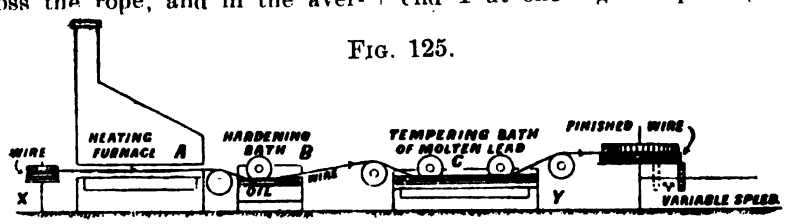
The demand for higher tensile strength for steel wire could be met, at present, only with an accompanying loss of ductility; from enquiry he had found that wire makers would not guarantee an increase of tensile strength above 120 tons per sq. in., without a corresponding loss in ductility of about one twist less for each ton added to the tensile strength of the rope. One of the tests for ductility was to demand a certain number of continuous twists, before breaking, in a wire which had a length of 100 times its own diameter. The following were some of the standards:—

Breaking Strain per sq. in. Tons.	Twists before breaking.
90	38
110	36
120	32

The tests for ductility were usually carried out by a machine 8 in. between the grips holding the piece of wire; this length of 8 in. was arrived at by multiplying 14 W-G. = ·080 in. by 100 = 8 in.; all torsional tests being proportional and related to this standard. There was generally too little consideration given to the torsional test, and too much to tensile strength; ductility was of the greatest importance, for the wire ropes when in use were turning round the whole time they were at work, in consequence of their spiral formation, and it was reasonable to assume were subjected to severe torsional strain when they were submitted to shocks.

There was a process in wire manufacturing called "patenting," which consisted in passing the wire through a heating furnace A, through a hardening bath of oil B, and finally through a tempering bath of lead C, the wire passing from the end X to the end Y at one regular speed (*see Fig. 125*).

FIG. 125.



This process annealed, hardened, and tempered the wire so regularly that its structure was homegeneous throughout, and a high tensile strength was attained. He suggested that the structure of the wire should be made to conform more to the conditions of working, by arranging some such plant as the above, but with the addition of simple speed variation gear at the end, whereby the wire could be passed through the apparatus at varying speed, to produce varying temper, and that the part of the rope near the hoisting drum could be made with the hard wire as taken from the draw bench, and the part of the rope near the cage could be tempered to a greater degree. The wire structure would then be graduated to meet the demands for ductility at the lower end, in cases where no balance or tail-ropes were used. There was at least one case on these fields in which very high-class wire hoisting ropes, having high breaking strain, caused much trouble and expense for some time; the difficulty was finally remedied by removing the rope with a high breaking-strain and substituting one which had a much lower breaking-strain but greater ductility.

There were also the important questions of high rope speed to be considered, and the effects of very rapid acceleration on ropes when in use. It had been well established under other conditions that steel, if allowed to have some period in which it might recover from its "fatigue," after having been subject to great stress, was able to withstand and support great loads; but that if the period allowed for change of condition, in which to recover from "fatigue," was insufficient, there was failure. Now it might be considered whether the high speeds, proposed to be adopted in deep level hoisting, would not be detrimental to the life of wire ropes, as well as to the shaft timbering, and whether rapid acceleration of speed would seriously affect them. Taking the case of ordinary hoisting cranes, it was often seen that ⅝-in. or ¾-in. ropes were not damaged, even when worked on drums of very small diameter, but at *low speeds*. Now let them examine the conditions on a mine headgear where the load was hanging for some time. The hard wire "set in" to the arc of the sheave, and then suddenly, and without time to recover from its "fatigue," there was a demand made upon it to "straighten-out" itself for the upward journey. It might be that in addition there was at the same time a lot of jerking going on. It might be said "There is plenty of reserve in the factor of safety." Let them agree that it might be so, but the shocks were so irregular, uncompensated and ungoverned, that it could not be a subject for surprise when the structure of the rope failed, altered, or become unreliable. Very high rope speeds, and very rapid acceleration, did not in any way tend to improve the conditions, unless other compensations were made.

In connection with the subject of sudden shocks conveyed to ropes, it was notable that while the factor of safety for chains (which were used in many cases where life and property were dependent on them) was considered by Lloyd's and the Admiralty to be sufficient if taken at 4, yet for wire ropes, which were of much more perfect and reliable structure, a factor of safety varying from 5 to 10 was demanded. It must, however, be admitted that ropes were abraded in use more than chains, but they were also examined more frequently. If the conditions on a railway were examined, it would be found that the drawbar pull of a locomotive with a mean weight of train of 770 tons was about 11 tons, an amount which was sufficiently near to the rope load in the hoisting problems now under discussion, that the cases might be compared. In the case of the railway every care was taken, at great cost, to relieve the drawbar of sudden strain by the provision of intervening springs. Could not this be attempted in hoisting? It would doubtless be said, "Springs have been already tried, and proved to be failures, through the difficulty caused by the time wasted in coming to rest at the end of the trip." This could be admitted as a real objection, but one which could be overcome by the adoption of better designed springs and connections than those hitherto used, or the sudden shocks and strains might be prevented by the adoption of a device on the engine similar to the Walker rings, whereby any jerk on the rope could be *prevented*, and thus give much greater confidence in the factor of safety adopted, and would allow the mine manager to calculate with some degree of certainty the reduced efficiency of the rope according to the actual wear or abrasion, which could be seen on simple inspection.

The factor of safety for a crank shaft was anything between 20 and 40 in a reversing engine, for the simple reason that there were shocks of such a formidable nature to be dealt with. This was only mentioned to show that the factor of safety was necessarily higher when the shocks could not be prevented. One of the chief troubles connected with the life of deep level hoisting ropes in the future, would be caused by the demand for high rope speeds for hoisting, unless some advance was made to prevent shocks being conveyed to the rope during hoisting. So long as high speeds were demanded, and so long as no apparatus, other than the structure of the rope itself, was provided to absorb or take up shocks, it could not be expected that the factor of safety for ropes would be reduced.

Steel wire was adopted for ropes, in lieu of iron wire, in order to strengthen and lighten them, and also to lessen the wear of the softer material, but he would suggest that the other extreme of unnecessary hardness of " hard-drawn " steel wire, had been, and might be, the cause of much trouble. It might be that the breakage at the lower ends of the ropes, and the consequent extra attention and expense for cutting off lengths, re-capping, etc., was caused by the fact that the rope was too hard drawn, and became crystalline in use, and that the remedy lay in further tempering. It might be objected that tempering would reduce the tensile strength of this lower portion, but it was possible that makers of ropes could meet the situation, as they had hitherto done so well, by not reducing the tensile strength more than necessary and by providing the most suitable *graduated structure to meet the varying strains* which existed in ropes when they were used without balance or tail-ropes.

In a paper read before the American Institution of Mining Engineers* Mr. Frank H. Probert pointed out that in certain tests made by him he found the tensile strength of wire ropes was reduced shortly after they were put to work, but that the strength in the course of a short time increased. He took note of the averages, and he found the strength slowly fell away from the first to the sixth month, and then increased gradually from the sixth to the twentieth month, after which the strength gradually decreased. He assumed that this was caused by a molecular rearrangement in the structure of the steel. He (Mr. Austin) suggested that, if such were the case, graduated and partial annealing might allow that the portion of rope required to bear shocks would be in a better condition to do so than if it were left " hard-drawn " as received direct from the wire-drawers' bench. Professor Undeutsch and other engineers of extensive experience had dealt with this matter, and they considered the lower portion of ropes to be the part most dangerous and liable to fracture. Many suggestions had been made to preserve the internal structure of the strands. Some European rope-makers advised galvanising the wires, but this method was one full of danger, and should not be countenanced. Even if the metal should adhere at first, the whole of the metal coating was split up into laminæ after a short period of work, and the last state of the rope was worse than if it had never been galvanized. He had found that the most satisfactory of all protective dressings was composed of three-parts of raw linseed oil and one part Stockholm tar; this was heated and frequently applied to the rope while hot, and allowed to soak into the interstices between the wires, after they had been thoroughly brushed and cleansed from all adhering filth. No lumps of any kind of foreign matter should be allowed to secure a lodgment on the rope, and every strand should be clean for perfect and complete examination. The remarks of Mr. Frerichs about the advantages alleged to be obtained by using taper ropes, and the reduction of the oscillations thereby, were very interesting; but he desired to point out that there would, theoretically, be more " nodes " in the length of the taper rope than in the thicker rope of uniform section. The shocks which really did the damage to ropes were set up through the solid, inflexible connection with the *huge, massive drums*, conical or cylindrical, and by the jerks experienced at various parts of the trip, which irregularity would be considerably reduced by working with tail-ropes, and by having flexible connections between the hoisting rope and the engine.

* * * *

He had inquired into the question of the effect of bending ropes round unsuitable

* " Trans. Amer. Inst. M.E.," vol. xxx, p. 1,020.

drums of small diameter, in consequence of the provision by Mr. Behr, in his Case IX, of a conical drum with the small end only 10 ft. diam., upon which a rope at least 1½ in. diam. had to be wound. The life of a rope was entirely dependent upon the amount of abrasion of the surface, the conditions to which it was subjected in use, to which the writer had already referred—the corrosive effects of acid water, and the means adopted to prevent the latter destroying the whole of the internal structure of the rope. Assuming that the greatest care was taken to prevent chemical deterioration, great damage was caused to ropes travelling at high speeds by bending them around, or winding them on drums or sheaves which had too small a diameter. This could not happen to a Whiting hoist fitted with sheaves of proper size, as there was not sufficient time for the ropes to become "set" or damaged.

If the conditions of the consulting engineer's office or the quiet drawing office could be obtained, and absolute certainty existed that the ropes used in hoisting would never be overstrained, many troubles in the engine room would be overcome; but while it was possible that an ordinary cylindrical or conical drum might at the start of the trip be jerked round, and thus wind up an overstrained rope, which would be kept at an unnecessary tension while on the drum for the whole of the trip, it would appear that the provision of frictional rope driving gear, and prevention of overstrain by slip rings, as in the Whiting hoist, were the best methods to obtain an automatic check. It was quite possible that the gear might be improved, but it appeared to him that sufficient credit had not been given to these excellent points of advantage in the Whiting system.

The strains set up in a wire, when subjected to longitudinal stress, were such as to cause the particles of which the material was composed to be lengthened; in other words, assume the particles to be normally circular in form, as in *Fig. 126*, when the hard drawn wire was not subject to any load; when the wire was subjected to stress, the particles would be changed from the circular form and would be elongated, as shown diagrammatically in *Fig. 127*. If the hard drawn wire were then subjected to bending, to such an extent that the condition known as "fatigue" existed, or should "permanent set" of the hard drawn wire take place, and should the operation be repeated frequently, at short intervals of time, then the lengthened particles (*Fig. 127*) of which the wire was composed would be unable to withstand the severe treatment, and the wires would break off "short," perhaps long before they had become abraded or worn down on the outside through legitimate wear. This condition was certainly not improved by the existence of a large number of brazed joints on the outside of the rope strands, as would be the case in long taper ropes made with brazed connections.

The curvature of the drum or sheave should be so designed that the resulting bending stress, added to the tension stress,

FIG. 126.

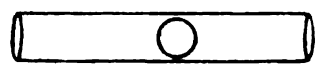
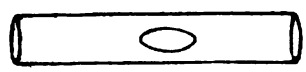


FIG. 127.



did not exceed the limit of elasticity of the material used. This bending stress was expressed in the formula.

$$K = 2{\cdot}06\frac{\frac{E\ a}{R}}{d} + C$$

in which

K = the required bending stress in pounds.
E = the modulus of elasticity 28,500,000.
a = the total sectional area in square inches of wires in the cross section of the rope.
d = the diam. of the individual wire used in the construction.
C = a constant used for the different numbers of wires forming the strand. The values of d and C for the different constructions of seven wires per strand and nineteen wires per strand respectively, were as follows:

7-wire strands (see Table 16).
$d = 1/9$th of diam. of rope.
$C = 27{\cdot}54$

19-wire strands (see Table 15),
$d = 1/15$th of diam. of rope.
$C = 45{\cdot}9$

For ropes made of wires varying in size, the diameter of the largest outside wire should be taken. The above formula had been supplied by the great authority on wire rope, Mr. Wm. Hewitt, Vice-President of the Trenton Iron Co., New Jersey, U.S.A.

Table 15.

Bending Stresses—Rope with 19-wire Strands.

Stresses in Pounds (due to bending).

Diam. of Rope.	Diameter of Sheave or Drum.																			
	6 in.	8 in.	10 in.	12 in.	14in.	16 in.	18 in.	20 in.	22 in.	24 in.	26 in.	28 in.	30 in.	36 in.	48 in.	60 in.	72 in.	84 in.	96 in.	108 in.	120 in.
In.																					
¼	1,801	1,390	1,131	965	827	726	654	586	535	495	455	423	398	338	250	200	167	144	126	112	101
5/16	3,308	2,568	2,098	1,774	1,536	1,355	1,212	1,096	1,000	920	852	795	742	621	468	376	314	270	236	210	189
⅜	—	3,776	3,094	2,620	2,273	2,006	1,796	1,626	1,485	1,366	1,265	1,178	1,102	924	698	561	469	403	353	314	283
7/16	—	—	5,551	4,546	3,951	3,494	3,132	2,838	2,594	2,389	2,214	2,063	1,931	1,620	1,226	986	824	708	621	553	498
½	—	—	—	6,609	5,755	5,096	4,573	4,147	3,793	3,495	3,241	3,021	2,829	2,376	1,800	1,448	1,212	1,042	913	813	733
9/16	—	—	—	—	8,337	7,393	6,642	6,029	5,519	5,089	4,721	4,403	4,125	3,468	2,630	2,118	1,773	1,525	1,338	1,191	1,074
⅝	—	—	—	—	11,565	10,270	9,237	8,392	7,689	7,095	6,586	6,145	5,759	4,847	3,680	2,967	2,485	2,137	1,876	1,671	1,506
11/16	—	—	—	—	—	13,360	12,027	10,936	10,027	9,257	8,597	8,024	7,524	6,201	4,818	3,886	3,257	2,802	2,459	2,191	1,976
¾	—	—	—	—	—	—	15,309	13,932	12,782	11,807	10,971	10,245	9,609	8,101	6,165	4,977	4,173	3,591	3,153	2,809	2,534
⅞	—	—	—	—	—	—	—	21,403	19,662	18,183	16,910	15,805	14,835	12,528	9,556	7,724	6,481	5,583	4,886	4,371	3,943
1	—	—	—	—	—	—	—	—	—	27,612	25,707	24,047	22,589	19,113	14,614	11,830	9,937	8,566	7,528	6,714	6,059
1¼	—	—	—	—	—	—	—	—	—	—	35,620	33,347	31,347	26,566	20,357	16,500	13,872	11,966	10,523	9,389	8,474
1½	—	—	—	—	—	—	—	—	—	—	—	—	42,036	35,683	27,400	22,239	18,713	16,153	14,209	12,682	11,452
1¾	—	—	—	—	—	—	—	—	—	—	—	—	—	48,109	37,028	30,096	25,350	21,897	19,272	17,209	15,545
1¼	—	—	—	—	—	—	—	—	—	—	—	—	—	61,238	47,229	31,436	32,403	28,008	24,662	22,030	19,906
1⅛	—	—	—	—	—	—	—	—	—	—	—	—	—	—	59,094	48,152	40,629	35,140	30,957	27,664	25,005
1½	—	—	—	—	—	—	—	—	—	—	—	—	—	—	74,565	60,844	49,919	44,476	39,203	35,048	31,689
1⅝	—	—	—	—	—	—	—	—	—	—	—	—	—	—	90,325	73,795	62,379	54,022	47,639	42,606	38,534
2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	88,409	74,795	64,814	57,183	51,160	46,285
2¼	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	125,387	106,265	92,203	81,428	72,908	66,002
2½	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	145,246	126,185	111,546	99,951	90,540

Table 16.

Bending Stresses.—Rope with 7-wire Strands.

Stresses in Pounds (due to bending).

Diam. of Rope.	Diameter of Sheave or Drum.																		
	10 in.	12 in.	14 in.	16 in.	18 in.	20 in.	22 in.	24 in.	26 in.	28 in.	30 in.	36 in.	48 in.	60 in.	72 in.	84 in.	96 in.	108 in.	120 in.	132 in.
In.																				
¼	1,864	1,572	1,359	1,196	1,069	966	881	810	749	697	652	545	411	330	275	236	207	184	166	151
9/32	—	2,120	1,834	1,616	1,444	1,305	1,191	1,095	1,014	943	882	738	556	447	373	321	281	250	225	205
5/16	—	—	2,620	2,311	2,067	1,869	1,706	1,569	1,453	1,352	1,265	1,060	800	642	537	461	404	359	324	294
⅜	—	—	—	3,949	3,536	3,201	2,925	2,692	2,493	2,322	2,173	1,822	1,377	1,106	925	794	696	620	558	508
7/16	—	—	—	—	5,561	5,039	4,607	4,243	3,932	3,664	3,440	2,878	2,178	1,751	1,465	1,259	1,104	982	885	806
½	—	—	—	—	—	7,073	6,470	5,962	5,528	5,153	4,836	4,053	3,070	2,470	2,067	1,777	1,558	1,387	1,250	1,138
9/16	—	—	—	—	—	—	9,414	8,701	8,053	7,510	7,036	5,915	4,486	3,613	3,025	2,601	2,282	2,032	1,831	1,667
⅝	—	—	—	—	—	—	—	—	11,232	10,480	9,823	8,267	6,278	5,060	4,238	3,646	3,199	2,849	2,569	2,339
11/16	—	—	—	—	—	—	—	—	—	—	12,508	10,535	8,008	6,459	5,412	4,657	4,087	3,641	3,283	3,059
¾	—	—	—	—	—	—	—	—	—	—	—	13,655	10,392	8,388	7,032	6,053	5,314	4,735	4,270	3,888
⅞	—	—	—	—	—	—	—	—	—	—	—	21,585	16,465	13,309	11,168	9,620	8,449	7,532	6,795	6,189
1	—	—	—	—	—	—	—	—	—	—	—	—	24,492	19,824	16,651	14,354	12,613	11,249	10,151	9,249
1¼	—	—	—	—	—	—	—	—	—	—	—	—	34,721	28,144	23,661	20,411	17 986	16,011	14,453	13,172
1½	—	—	—	—	—	—	—	—	—	—	—	—	—	38,472	32,374	27,945	24,582	21,942	19,814	18,062
1¾	—	—	—	—	—	—	—	—	—	—	—	—	—	—	42,962	37,110	32,661	29,164	26,344	24,021
1¾	—	—	—	—	—	—	—	—	—	—	—	—	—	—	55,595	48,054	42,314	37,799	34,155	31,151

To enable mining engineers to readily find the amount of stress to which ropes were subjected on drums in general use from 8 ft. to 16 ft. diam., he had calculated the following Table 17 (p. 32), and plotted out the curves on a diagram (*Fig. 128*, p. 31). From the Table and the Diagram, the relationship between diameter of drum and diameter of ropes and the bending stress could be quickly seen. Now from Table 17 it would be seen that with an ordinary rope, $1\frac{1}{2}$ in. diam., made up with 6 strands, each strand composed of 19 wires, and used on a drum 10 ft. diam., as proposed by Mr. Behr, there were the following stresses:—

FIG. 128.

DIAGRAM of Rope Stresses, in Ropes from $\frac{3}{4}$ in. to $2\frac{1}{4}$ in. diameters, when used on Drums or Sheaves from 8 ft. to 16 ft. diameters.

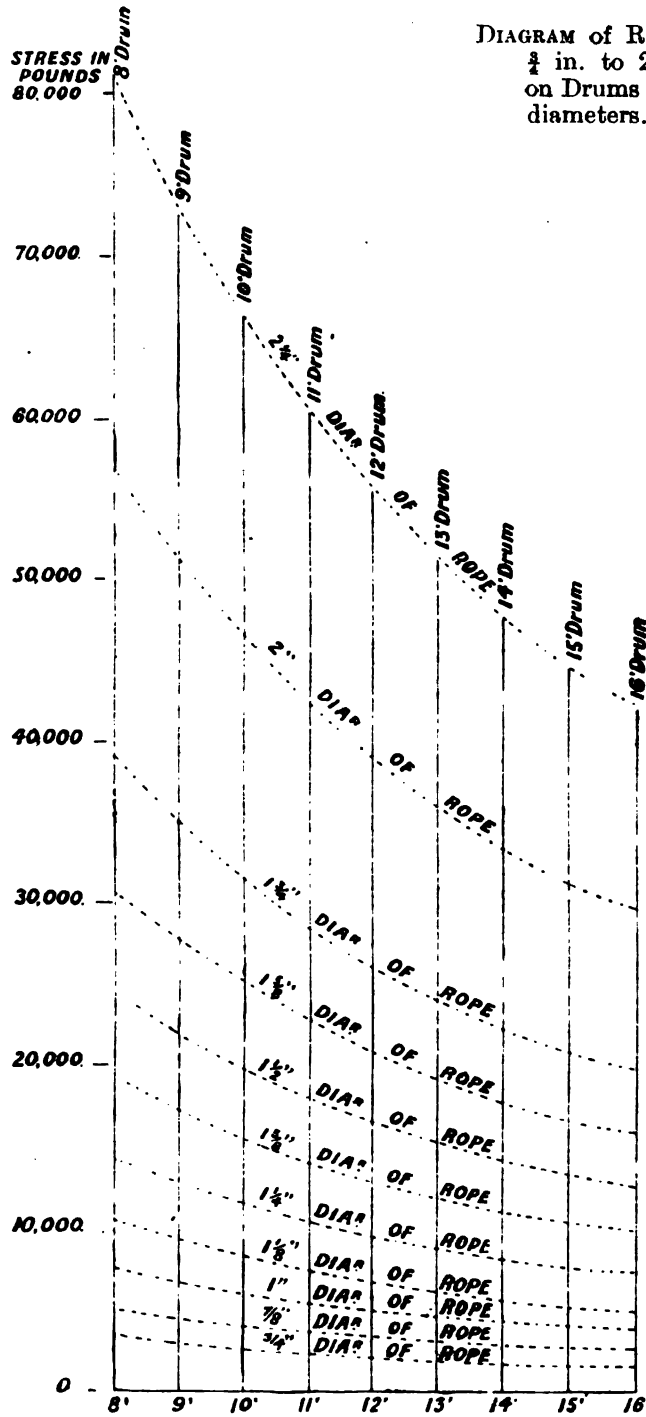


Table 17.

Bending Stresses and Rope Strengths—6 Strands, 19 Wires in each Strand. Centre Core of Fibre.

Approximate Circumference of Rope.	Approximate Diameter of Rope.	Approximate Weight per Fathom.	Approximate Weight per Ft.	No. of Wires.	Diameter of Wires.	Sectional Area of each Wire.	Total Sectional Area of Wires in Rope.	Bending Stress in Wires when used on Drums ("K"). Diameter.								Approximate Breaking Strain at 120 tons or 268,800 lbs. per sq. in.	Approximate Breaking Strain at 100 tons or 224,000 lbs. per sq. in.
								8 ft.	9 ft.	10 ft.	11 ft.	12 ft.	13 ft.	14 ft.	15 ft.		
In.	In.	lbs.	lbs.		In.	In.	In.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	bs.	lbs.	lbs.	lbs.
2¼	(·750 ¾)	5·34	·89	114	·050	·0019635	·223839	3,158	2,809	2,534	2,307	2,117	1,957	1,819	1,699	1,594	60,176	50,139
2¾	(·875 ⅞)	7·20	1·20	114	·058	·0026420	·301188	4,886	4,371	3,943	3,591	3,298	3,050	2,834	2,648	2,484	80,959	67,466
3	(1·000 1)	9·48	1·58	114	·067	·0035256	·401918	7,528	6,714	6,059	5,520	5,072	4,688	4,360	4,076	3,827	108,035	90,029
3¼	(1·125 1⅛)	12·00	2·00	114	·075	·0041787	·476371	10,523	9,387	8,474	7,303	6,711	6,205	5,769	5,391	4,862	128,048	106,707
4	(1·250 1¼)	14·70	2·45	114	·083	·0054106	·616808	14,209	12,682	11,452	10,445	9,595	8,878	8,260	7,723	7,249	165,797	138,164
4¼	(1·375 1⅜)	18·00	3·00	114	·092	·0066480	·757872	19,272	17,209	15,545	14,155	13,011	12,044	11,205	10,578	9,835	203,715	169,763
4¾	(1·500 1½)	21·10	3·55	114	·100	·0078540	·895356	24,662	22,030	19,906	18,162	16,689	15,446	14,368	13,430	12,613	240,671	200,559
5	(1·625 1⅝)	24·90	4·15	114	·108	·0091609	1·044342	30,957	27,664	25,005	22,824	21,004	19,451	18,104	16,940	15,907	280,719	233,933
5¼	(1·750 1¾)	29·10	4·85	114	·116	·0105680	1·204752	39,203	35,048	31,689	28,190	25,952	24,044	22,397	20,961	19,699	323,837	269,864
6¼	2·000 2)	37·80	6·30	114	·133	·0138929	1·583790	57,183	51,160	46,285	42,303	38,912	36,023	33,559	31,389	29,482	425,722	354,769
7⅛	(2·250 2¼)	48·00	8·00	114	·150	·0176715	2·014551	81,428	72,908	66,002	60,309	55,527	51,400	47,885	44,785	42,092	541,511	451,259

$$K \quad 2{\cdot}06\frac{R}{d} + C$$

$$\frac{E\ a}{}$$

E = 28,500,000 modulus of elasticity.

a = Total sectional area of wire in sq. in.

R = Radius of bend in inches.

d = Diameter of each wire in inches = 1/15th diam. of rope.

C = 45·9.

The figures for Mr. Behr's Case IX would then be as follows:—

I. Bending stress (Table 17		19,906 lb.
II. Load (as per Mr. Behr)	Case IX	10,000 ,,
III. Rope weight (do.)	do.	18,676 ,,
IV. Skip weight (do.)	do.	6,600 ,,
Total stress		55,182 lb.

The total stress	55,182 lb.	× Factor of Safety of	4 = 220,728 lb.
,,	55,182 ,,	× ,,	5 = 275,910 ,,
,,	55,182 ,,	× ,,	6 = 331,092 ,,
,,	55,182 ,,	× ,,	7 = 386,274 ,,
,,	55,182 ,,	× ,,	8 = 441,456 ,,
,,	55,182 ,,	× ,,	9 = 496,638 ,,
,,	55,182 ,,	× ,,	10 = 551,820 ,,

Now it would be observed that the approximate breaking strain in Table 17 was 240,671 lb., hence it would be seen that the remedy was to be found in increasing the diameter of the coned drum, or that of the rope, if a factor of safety of 6 or 7 were demanded. In this calculation the highest quality of steel rope has been taken, viz., 120 tons breaking strain per sq. in., but it was always better practice to adopt a lower figure and to increase the ductility of the rope.

The size of drum at the small end of the cone in one of the most notable examples, viz., in the conical drum engine installed by E. Tomson for the Harpener Mining Company, Limited, at Preussen, Dortmund (the engine exhibited at the Düsseldorf Exhibition, and shown by Mr. Jennings), was 108 times the diameter of the rope. If Mr. Behr adopted this practice, the diameter of his drum at the small end, in his Case IX, should be about 13 ft. 6 in., instead of 10 ft.; assuming he used a rope 1¼ in. diameter. The size of the wires in the ropes used on the coned drum for the above-named engine at Preussen, Dortmund, was 2·6 mm., or No. 12 W. G. diameter.

Mr. Behr had assumed the small end of coned drums at too low a figure, and it would be more advisable to increase the diameter of drums in Case IX than to run the great risk of damage to the ropes. It was far better to admit to the full, all the disadvantages of the conical drum system, and to follow the sensible practice of which the Harpener Mining Company's installation was a good example.

Mr. D. Gilmour had called very timely attention to one most important factor in all deep level hoisting operations—the human factor. On all sides, any one who has had experience of the Witwatersrand, could see the way in which the leaders and directors of the gold industry had endeavoured to train up, and get the best possible men from all parts of the world to assist in the development of the country, and it was to be hoped that sterling reliable men would be available as deep level engine drivers, when there was a call for them. No reflection was cast upon the present day drivers; but it must be admitted by all that the problem of outcrop mine hoisting was very different to that of hoisting from 6,000 ft., and everyone who had the opportunity, should train men to fill these highly responsible positions. It was surely not an extreme statement to make, that all the calculations and energies of our great mathematicians and engineers were nullified unless the engine driver was absolutely reliable, or unless it was made impossible for him to exceed the conditions laid down for safe working in the deep level shafts.

* * * *

One great argument put forward by conical drum advocates, was that the inertia of the heavy drums with taper ropes was compensated by the lower inertia of the lighter taper rope; but this, roughly stated in such a manner, was not supported by fact or reason, for, if the enormous mass in the rotating drums was compared with the relative small mass of, or the small difference in weight between, the taper ropes and ropes of uniform section, the difference was comparatively so small that this rough form of

stating the argument was of very little value.

The life of a rope of uniform section, first for hoisting vertically and next for hoisting on the incline in the mine, and finally as tail-rope, would be quite double that of the taper rope. This fact should induce those who were such ardent advocates of taper ropes to pause and examine whether, with the smaller steam cylinders needed for starting and for balanced ropes of uniform section, the numerous economical advantages, as shown by Messrs. Hennen Jennings and A. M. Robeson, and obtainable by use of the Whiting system, did not surpass all the alleged advantages of other systems.

It was quite impossible to definitely discuss the increase of inertia in exact terms, unless actual weights and sizes were determined by the advocates of conical or other types of drum.

* * * *

(*Page* 264.] Mr. E. J. Laschinger said that the discussion on Mr. Behr's most valuable and opportune paper had brought forward the question of the best kind of hoisting ropes that should be used.

This was a feature of the deep level winding problem on which they had very little experimental data, and much less statistical information about ropes as used in practice.

As far as he was aware no general standard by which to measure and compare the duty of hoisting ropes had been adopted. Could an engineer state beforehand the probable life of a wire rope, or the probable number of tons of ore it would hoist from a given depth before it was worn out? As this question was certainly a matter of importance it would repay careful investigation.

The duty of ropes was variously given in mile tons, ft. tons, or kilogrammetres; but these measures of useful work done by a rope were obviously comparable only when hoisting was done from the same depths, and with the same loads.

There were certain factors involved which determined the life and duty of a rope, some of which could be taken account of in a formula, and other factors which were not capable of mathematical treatment. The whole subject being so involved, it was not surprising that it had received little attention. There was no doubt, however, that if some standard were adopted for comparing the life and duty of wire ropes, it would prove highly useful, even if it were not strictly accurate and final.

The final basis of comparison which appealed to the mine owner or shareholder, was the useful work done per cost of rope, or stated as a formula:

$$\text{Useful duty of rope} = \frac{\text{Ore hoisted x depth.}}{\text{Cost of rope.}}$$

To the engineer this equation resolved itself into a different form:

$$\text{Useful duty of rope} = \frac{\text{Tons of ore hoisted x depth.}}{\text{Weight of rope.}}$$

but the actual work done by the rope was greater than the useful work. The dead weight of the skip must be added.

To the engineer the best rope was the rope which would hoist the total load the greatest number of times. The weight of the rope was dependent upon (1) the depth of the shaft, (2) the factor of safety chosen, and (3) the design of the rope, whether of uniform section or taper. The total duty of the rope was increased as more work was got out of the weight of the material in the rope.

After a careful consideration of all the conditions, he had arrived at the conclusion that a fair standard for comparing ropes in conjunction with the winding arrangements of which they formed a part was given by the following formula:—

$$\text{Duty of rope} = \frac{\text{Total weight hoisted in tons x depth in ft.}}{\text{Weight of rope in lbs.}}$$

This gave ft. tons of work done per lb. weight of rope. The formula would slightly favour moderate depths as against great depths. The duty of the rope would vary with different factors of safety—the factor of safety with the rope in motion being meant. For want of a better name, he would call this the dynamic factor of safety, to distinguish it from the static factor of safety calculated on the basis of load stresses. The dynamic factor of safety included stresses, due to bending, acceleration and other induced stresses, as well as the static load stresses. The elements dependent upon different designs of engines and sheaves were

thus practically eliminated. The factors not eliminated were those due to the manipulation of the engine driver in handling his engine, the action of acid or mineral waters on the rope, the care bestowed on the rope, accidents, and such other factors which were outside the province of mathematics. This formula applied to the hoisting of different loads from different depths, because as the load and depth increased, the work done by the rope increased in the same proportion. This formula for determining the duty of ropes would, of course, have to be applied separately to vertical and inclined shafts, because the wear on the rope was much greater in inclined shafts.

If this formula were applied to taper ropes and ropes of uniform section alike, it should show which kind of rope was preferable.

It was interesting in this connection to see if the contention of Mr. Hennen Jennings and Mr. Robeson in preferring ropes of uniform section to taper ropes were upheld in practice. Fortunately he was able to give some figures on ropes as used in actual practice.

Bach, in his *Maschinen Elemente*, 8th edition, vol. i, p. 547, gave the particulars of taper ropes as used in the Kaiser Franz Josef Shaft at Przibram. These ropes, according to C. Habermann (whose report appeared in the *Oesterreichische Zeitschrift für Berg-und Hüttenwesen*, 1895, p. 193), "have given every satisfaction."

For comparison with these taper ropes they had, through the kindness of Mr. P. A. Robbins, of the De Beers Consolidated Mines, Ltd., Kimberley, statistics of the ropes of uniform section as used with the Whiting hoist.

In the table of comparisons submitted herewith (Table 18), he had assumed the material of all the ropes to be of the same ultimate strength, as they were all made of best crucible or plough steel. In calculating the bending stresses Bach's formula had been used throughout.

From a study of the table it would be seen that the taper ropes and ropes of uniform section gave practically the same duty for the same dynamic factor of safety. The result would be greatly in favour of the taper ropes if the tail-rope were taken into account in the calculations on the Kimberley ropes; a tail-rope, even if it were an old rope, must cost something; the expense of putting it on was in itself a considerable item.

What struck him forcibly was that the length of time the rope was in use seemed to have little or no effect on the duty of the rope. The hoisting at Kimberley was done at greater speed with the result that the ropes wore out in a shorter time (about six months average), while the Przibram ropes lasted 36 months on an average, the duty in both cases remaining about the same. It would be well, therefore, in cases where acid waters might act on a rope, to do the hoisting at as great a speed as possible, so that the rope would be worn out before the acids could appreciably weaken it by corrosion.

Another point brought out by this table was that the duty of a rope decreased rapidly with a slight decrease in the dynamic factor of safety; with a decrease in the dynamic factor of safety from 5·4 to 4·6 the duty of the rope decreased 66%. The moral of this was, that it was highly inadvisable to work with very low factors of safety. The cost of replacing a rope in a very deep shaft was a very considerable item, and it might be advisable to have slightly bigger engine cylinders, and work with a higher factor of safely, than to decrease the factor of safety and lessen the duty and life of the rope. A balance should be struck between cost of extra steam consumption and extra cost of ropes.

It seemed that at Kimberley only one light rope with a low factor of safety was used—8·7 static factor, as against an average of 10·8. Presumably it was found unsatisfactory, owing to its low duty and short life, as the next rope put on had a static factor of safety of 11·1.

TABLE 18.—DUTY OF WIRE ROPES.

	Przibram Averages.	Kimberley Ropes.		
		Average of Four Ropes.	One Rope.	One Rope.
(1) Depth of Shaft	4,100 ft.	1,520 ft.	1,520 ft.	1,840 ft.
(2) Useful load	4,409—1,852 lb.	9,600 lb.	9,600 lb.	9,600 lb.
(3) Weight of skip, or cage and cars	3,748—2,425 lb.	7,280 lb.	7,280 lb.	7,280 lb.
(4) Kind of wire rope	Taper	Uniform section.	Uniform section.	Uniform section.
(5) Diam. of rope	1·161 in.—0·925 in.	1¼ in.	1¾ in.	1¾ in.
(6) Weight of rope in use	5,877 lb.	8,897 lb.	6,699 lb.	10,025 lb.
(7) Static factor of safety	6·9	10·8	8·7	11·1
(8) Dynamic factor of safety	5·3	5·4	4·6	5·3
(9) Duty of rope based on useful load	58,782 ft. tons.	78,480 ft. tons.	26,604 ft. tons	60,923 ft. tons.
(10) Duty of rope based on total load	127,000 ft. tons.	138,000 ft. tons.	47,000 ft. tons	107,000 ft. tons
(11) Life of rope, months	36·6	5·8	1·4	4·0

NOTES.—Przibram rope runs on a cylindrical drum 12 ft. 6 in. diam.

Kimberley rope runs on Whiting sheaves 10 ft. diam.

Weight of tail-rope not included in total weight of a rope in use in calculating the duty of the Kimberley ropes.

If the Kimberley Whiting hoist is running without the coupling rods, the above figures relating thereto are not correct, for then the dynamic factor of safety for the ropes of uniform section will probably be much greater than the figures given.

Again, in a rope that was worn out, the interesting and instructive part to see was that section of the rope which actually failed through breaking of some wires or excessive wear, and not a section of the rope, which had the whole rope been like it, might still be used to do the hoisting.

They had also been favoured with some figures regarding the cost per ton hoisted per 100 ft. length of rope at Kimberley, and compared with the cost of tapered ropes at Przibram. It might be remembered, in this connection, that the Przibram ropes worked with a static factor of safety of 6·9 and the Kimberley ropes with an average factor of 10·8.

At Przibram also the dead weight of cage and car hoisted was greater than the useful load, while at Kimberley the ore weighed more than the skip. The cost of the hoisting per 100 ft. length of rope at Kimberley should be 0·0046*d*. per load of 1,600 lb., not per ton, as stated by Mr. Robeson. When the rope with a static factor of 8·7 was used at Kimberley the cost per 100 ft. promptly rose to 0·014*d*. per load, or 0·0175*d*. per ton, as against 0·011*d*. per ton at Przibram with factor of safety as low as 6·9. What would be the probable cost if a factor of safety of 6·9 were tried with the Whiting hoist?

One thing which the opponents to the use of taper ropes seemed to have looming largely in their minds was, that the use of tapered ropes involved a new and untried method of rope construction, and that it would be risky to introduce them on the Rand as an experiment. Mr. Robeson described the rope of uniform section as "a perfect structure," thus implying that the tapered rope was an "imperfect structure." As a matter of fact, tapered steel wire ropes had long since passed the experimental stage; they had been manufactured and used in the lead and silver mines at Przibram continuously for 25 years. That they had not been used more extensively, was due to the fact that their superiority over ropes of uniform section was not so evident at moderate depths (up to, say, 3,000 ft.), which was the limit of most mining enterprises, to date, with a few exceptions. When they come to treat of depths ranging from 3,000 to 6,000 ft., or even beyond, the tapered rope had its legitimate field opened up. From the duty table which he had compiled, it would be seen that the tapered ropes were not one whit behind the ropes of uniform section, and that they were just as strong and reliable in the details of their construction.

Brazed joints in the wires had come in for some acrimonious criticism, yet it was a fact that there was probably not one wire rope on the whole Witwatersrand which had not brazed joints in its wires. The "electric couple" argument was pretty, but more fanciful than real. It appeared that Mr. Kenneth Austin did not understand how a brazed joint in a wire was made. The two wires were not butted squarely together and brazed between, so that the strength of the joint was equal only to the tensile strength of brass of cross-section equal to that of the wire. The joint was made by cutting the two wires off on a slant so that the length of the faces to be joined was at least ten times the diameter of the wire. These elliptical faces were then brazed together.

Assuming that the long axis of the face of the joint was ten times the diameter of the wire, and that the tensile strength of brass was 15 tons per sq. in., the strength of the wire 120 tons per sq. in., then the area of the elliptical face was—

$$\frac{10\,\pi\,d^2}{4}$$

The cross-section of the wire was—

$$\frac{\pi\,d^2}{4}$$

or the face of the joint is ten times the cross-section of the wire.

The strength of the joint would therefore be 150 tons per sq. in., and the strength of the wire was only 120 tons per sq. in., the joint being 1¼ times as strong as the wire. This bore out the statement of all wire rope manufacturers that when a joint was tested the test piece generally broke in the wire itself, and not in the brazing of the joint.

With regard to bending stresses produced in a rope by being wound over a drum or sheave, many formulæ had found circulation, and the results as given by different authorities differed so much that Hrabák had appropriately described this subject as the "stumbling block" to engineers. In a

recent work called *Die Drahtseile*, 1902, by Josef Hrabák, two chapters were devoted to a most lucid and conclusive mathematical and practical treatment of this dark subject. He showed clearly how the elasticity of the wire, as it was in the rope, depended upon the spiral form it assumed, and he gave the scientific method of calculating the bending stresses; everyone knew that the elasticity of the wires in the rope depended upon the twist, but no one heretofore seemed to have investigated the bending stresses mathematically on this basis.

For an average angle of twist of 18° Hrabák gave the following figures for new ropes—

I for a twisted strand.

II for a rope of strands twisted round a centre line.

III for a cable of ropes twisted round a centre line.

	Modulus of elasticity of the rope as a whole.	Modulus of elasticity of the wires in the rope.
I	·6 E	·6636 E
II	·36 E	·44 E
III	·216 E	·2918 E

E is modulus of elasticity of a straight wire.
Note.—These figures were for ropes without hemp cores; with hemp cores ropes were more elastic, i.e., E was lower.

This proved what everyone knew, that in proportion to their diameter a rope was more flexible than a strand, and a cable more flexible than a rope.

The best formula for calculating the bending stresses was the one given by all the best recognised authorities, Reuleaux, Unwin, Bach, Hrabák, and others.

$$p = E \frac{d}{2R}.$$

" p " being the stress, " d " the diam. of wire in inches, R the radius of sheave in inches, and E the modulus of elasticity of the wire as it was when in the rope. This was found by multiplying the modulus of elasticity of a straight wire by the proper fractional coefficients as given by Hrabák.

Formulæ which introduced empirical coefficients, or formulæ which were empirical in form, were to be avoided.

The general results of Hrabák's investigation showed that the bending stresses were much lower than those given by most formulæ, and were only about half of the figures given in Mr. Austin's table. Anyone interested in this subject should study this valuable and interesting book on wire ropes.

The formula given by Bach gave practically the same results as were obtained by Hrabák's method. The subject of bending stresses was not a paramount one in a well-designed plant; the results of numerous experiments showed that if a wire rope were bent over a sheave to the breaking point of the rope, the fracture on the rope nearly always took place in the straight part of the rope and not on the sheave.

Mr. Robeson deserved great credit for attacking the differential stresses problem in a practical manner by actually measuring them in a few cases. It was one of the dark problems connected with the Whiting system, for there were no differential strains in a drum winder. It appeared from the experimental results that these strains, which did no useful work but strain the rope needlessly, were very considerable and reduced the factor of safety. Mr. Robeson gave it as his opinion that (as in his experiments the stresses were greatest in the smallest drums) with 16 ft. diam. drums the differential strains would become negligible. What this latter statement was based on was unfortunately not stated.

This differential strain was produced because the rope wound over two sheaves which being coupled together must move with the same angular velocity. As it was impossible in practice to make two sheaves (especially large ones) of exactly the same diameter, the natural result was that one sheave wound on more rope than the other, consequently the rope was stretched between the sheaves, thus inducing the differential strains.

Let R = radius of one sheave or groove.
R + x = radius of the other sheave or groove.
l = distance between centres of sheaves.
E = modulus of elasticity of the rope.

Then the stretch of the rope due to the different diameters of the sheaves while any fixed point of the rope moved from the point of initial contact with the larger sheave to its point of final contact was—

$$\pi (R + x) - \pi R = \pi x$$

The length of rope in white this stretch occurred, if there were no slip, was—

$$l + \pi R$$

The differential strain in the rope for the first lap was therefore—

$$P = \frac{\pi x}{l + \pi R} E$$

For $x = \frac{1}{32}$ in. $R = 8$ ft. and $l = 18$ ft.

$E = 20{,}000{,}000$ for a rope in use for some time.

$P = 3{,}800$ lb.

This differential strain continued in the rope until the second lap was reached, when the same amount was again added.

Strain on second lap = 7,600 lb.

On the third lap a similar amount would be added, but as the rope where it left the drums finally could have no differential strain, it would crawl back to a certain extent on the periphery of the third groove and ease the strain there.

These results, as calculated theoretically on assumed data, tallied in their relative proportions with Mr. Robeson's experiments in a general way, the strain being a maximum on the middle lap. The actual strains as they occurred in running, however, could not be calculated, because it would be so difficult to get exact data to figure from. The theoretical investigation, however, showed that the strain would continue to exist, and would be high no matter what the diameter of the sheaves. The larger diameters of the sheaves reduced the differential strains only in so far as they gave a longer piece of rope to stretch. Therefore, until Mr. Robeson could procure two sheaves with grooves of exactly the same diameter to a hair's breadth, he could not get rid of differential strains. With carelessly constructed driving sheaves the differential strains would be very high.

[*The remainder of Mr. Laschinger's contribution referred to the other subject matter of the paper.*—ED.]

Mr. G. H. THURSTON *made the following remarks in the course of his contribution to the discussion:*

(*Page 281*) The bending of the ropes was also responsible for a percentage of the friction in a winding engine. That power was required to bend a rope was well known, and part of it was exerted in overcoming the friction of the wires sliding upon each other. The effect of this could be seen by opening up a rope that had been running over small pulleys for any length of time. The inside wires would be found to have worn at the point where they were in contact with adjoining wires, which resulted in a decrease of the sectional area of the rope, thereby reducing its strength, as represented diagrammatically in *Fig. 142*.

FIG. 142.

Diagram representing scorings on an inside wire of a worn wire rope.

This scoring action was also referred to by Mr. Laschinger as an evidence of the internal work done in a rope. From this it was evident that the fewer the number of pulleys that a rope passed over the less would be the loss due to friction, and the longer the rope would last.

In the case of the Whiting hoist at the Geldenhuis Deep, the number of times that the rope was bent and straightened after it came out of the shaft until it again entered the shaft was 15. With the drum hoist at the Ferreira Deep each rope was bent and straightened twice only. A tail-rope was used at the Geldenhuis Deep, but not at the Ferreira Deep; notwithstanding these differences, the percentage of efficiency of the drum over the Whiting hoist, according to Mr. Wood's calculations, amounted to less than one-quarter of 1%.

Mr. H. LEUPOLD, concluding his discussion, spoke on the subject as under:—

(*Page 287*). Before concluding he would further remark, with regard to the strength of brazed joints, that wires were not butted and soldered end to end, but that the joint usually had a length of five diameters. In no cross section would there be found more than a thin line of brass, whilst the total soldered surface would be about four times the cross section of the wire. For 120-ton wire this certainly showed a reduction of strength at the brazed joint to

$$\frac{4 \times 15}{120} = \cdot 5$$

of the original; but it should be borne in mind that these joints could be and were distributed over a considerable length, thus obtaining to the full the splicing effect which Mr. Behr had mentioned elsewhere.

A lengthy experience with ordinary steel wires, ½ in. diam., used for aerial tramways in stretches up to 6,000 ft. and as much as 1,300 ft. difference in level between anchorages, in a region of hot summers and very severe winters, showed him that the wire never broke at the brazed joints. In this case, however, the ultimate strength of the wire was no more than 40 tons per sq. in., and this was why the soldered surface at only 15 tons per sq. in. (but four times larger) was necessarily stronger. The cross section at the joint was also stronger owing to a slight overlapping of the chamfered ends, but, had the strength of the joint been to that of the wire as 15 to 40 only, the breakages would inevitably have occurred there.

From these considerations it would appear that brazing the wires composing a rope did not materially weaken it, and that this objection to taper ropes was not of great importance.

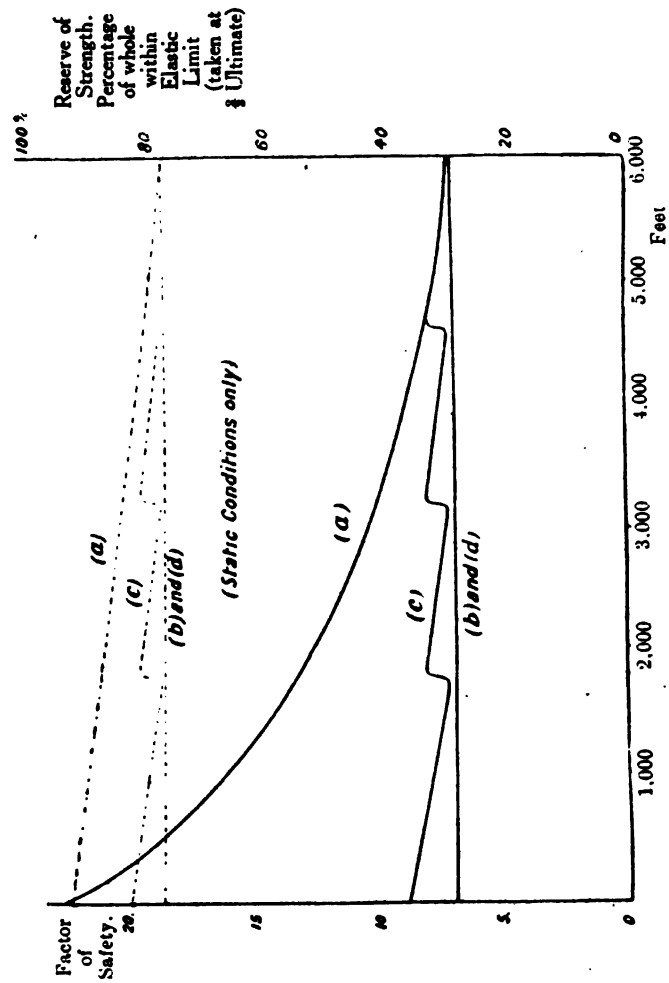
Mr. Edwd. Goffe (*extract*) (*Page 292*). The fact that taper ropes had been in use at Przibram for twenty-five years did not prove them to be other than imperfect structures, and it was not evident how a rope formed of wires of varying diameter in parts could be compared, as to perfection of construction, with one in which the wires were of the same size. At every spot where a brazed spot occurred, and the wire underwent a reduction in diameter (and this, according to Mr. Behr, was at about 16 ft. intervals), there must be some irregularity in the laying, and therefore some unequal straining and wearing of the wires. He had heard that it was the practice to lay short lengths of fine wire in the rope to make up these irregularities, and to some extent prevent the unequal straining. Could this be called perfect rope construction?

Mr. J. A. Vaughan *dealt widely with the subject of wire ropes in the course of his contribution, as follows:—(Page 313).* Again, it would appear that the introduction of the tail-rope for deep level winding was also a new departure, and the question whether this appliance provided a safe and efficient method of balancing, when hoisting from great depths had to be seriously considered.

The bad effect of a tail-rope in the lower stages of the deep level hoist was not appreciable; but in the progress upward, it had to be noted that its weight, gradually increasing, automatically prevented any increasing of the reserve of strength in the rope. An increase of the reserve of strength in the rope during the upward movement was a most desirable accompaniment to the winding, especially when the surface was being approached, for the resilience of the rope was decreasing, and the kinetic shocks incidental to retardation had to be encountered. Such increase was most desirable if the reserve of strength at the extreme depth had not been fixed unnecessarily high.

These remarks supplied a context to the extract quoted by one member from the results of Professor Undeutsch's researches. He showed, later on, how, in the design of the sectionally tapered rope an effort had been made to comply with the requirement just mentioned, and in which provision the theoretical tapered rope was lacking.

FIG. 146.



The above diagram, *Fig. 146*, showed the rate of change of the *statical factor of safety*, and also of the *reserve of strength* of a winding rope during a hoist from 6,000 ft. to the surface, for the following cases: (*a*) with a rope of uniform section; (*b*) with a rope of uniform section and tail-rope; (*c*) with a sectionally tapered rope, as suggested by Mr. Behr; (*d*) with a theoretical taper rope. The conditions, as far as loads, grade of steel, factor of safety, etc., were concerned, being the same as in Mr. Behr's Case IX.

It would be noticed that, considering statical conditions only, while there was no change in the value for factor of safety or for reserve of strength in the cases (*b*) and (*d*), in case (*a*) there was a very great increase, and in case (*c*) there was a variation which allowed of a moderate and increasing addition during the last 1,800 ft. of the upward trip.

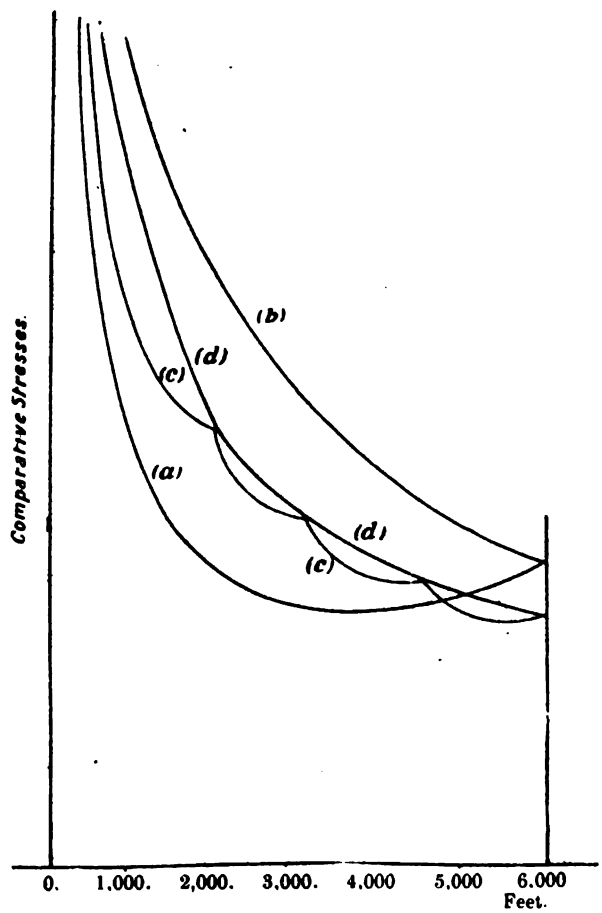
The effect of these variations of the reserve of strength in the rope in moderating the stresses, occasioned by kinetic conditions, might be capable of exact calculation, but it did not yet appear to have been dealt with satisfactorily. Approximations could, however, be made. It was evident, for instance, that in case (*b*) during the upward trip, though the mass in motion remained constant, the length, and with it the elasticity, of the rope from pit-head sheave to skip was gradually lessening, so that the stresses occasioned by a sudden acceleration or retardation occurring at any stage of the trip would increase as the hoist progressed.

Without attempting to explain or to justify certain calculations that had been made, he wished to invite attention to the results, which were shown in a diagram (*Fig. 147*). They were intended to represent an approximation to the relative values of the stresses that might be occasioned by the maximum possible theoretical kinetic shock, such as that due to the sudden stopping of the winding-drum, this event being supposed to occur at the various stages of a hoist of 6,000 ft., (*a*), (*b*), (*c*), (*d*), being the same as in the previous case.

In cases (*a*) or (*b*), it was possible, if sheave-winding were resorted to, that the stresses might be considerably reduced owing to slip; but, on the other hand, if sheaves of small moment of inertia were used, the maximum possible kinetic shocks might be more nearly obtained, than in the case of a winding plant using drums of necessarily larger moments of inertia.

While it was quite possible that the views held at present, regarding the proper factor of safety for winding ropes, might be somewhat revolutionised when the full safety effect of the elasticity of a long vertical rope was satisfactorily ascertained, even now it might be reckoned a sound deduction that the use of a tail-rope for high speeds and great depths was a mistake, and that a rope which combined the advantages of the uniform section type and the taper type was the best suited to meet all conditions.

FIG. 147.



In the Whiting hoist, as designed for the depth of 5,500 ft., and using a tail-rope, the load would approach the surface to within 400 ft. at a velocity of about 60 ft. per second, with a factor of safety in the rope (considering statical conditions only) of 6. This constituted quite a new departure in winding practice. The corresponding figure for the Kimberley Whiting was 9 or 10, he believed; in the average drum-hoist on these fields (without tail-rope) it was, say, 10; and at the Geldenhuis Deep, where a tail-rope was used, it was about 8. In the case of the Red Jacket Shaft a statical factor of safety of 5, when the load of 4 tons was at the bottom of the hoist, would be changed to over 10 when the load was near the surface.

There were, as far as he knew, no mitigating circumstances connected with the disparity pointed out, and the accusation of prejudice cast at those who knew these facts, and therefore condemned the use of tail-ropes for deep levels, appeared to be hardly justified.

It has been asked, how the sectional areas of wire at various points of Mr. Behr's sectionally tapered rope compared with the corresponding section of the theoretical taper rope.

By the use of the usual formula applicable to rods tapering according to the logarithmic curve, they could readily obtain values for the sectional areas at the ends of various lengths of a 6,000-ft. theoretical taper rope.

Table 21 showed the comparison between a rope of the above class and the rope used by Mr. Behr in Case IX, the load and other conditions being the same.

It would be seen that in the case of Mr. Behr's sectionally tapered rope, owing to the designed differences from the theoretical taper, there was for a statical factor of 7 at the extreme depth, a factor of safety of about 9 near the surface.

Comment was unnecessary in view of the previous remarks.

The objections raised to the construction of sectionally tapered ropes had not been backed up by the presentation of any statistics of the failure of such ropes, or of ropes of uniform section, at the brazed joints.

Some failures had doubtless occurred, and particulars regarding them would be very valuable at this juncture, as it was most probable that brazed joints would have to be resorted to in the wires of all ropes, whether of uniform section or tapered, for use in the deep levels; and, if the causes of past failures could be ascertained a remedy might be found for the defects.

In connection with the calculations for sectionally tapered ropes, and other ropes, the following formula might be found useful:

$$d = 1{\cdot}615 \sqrt{\frac{W}{f_t - 2.125N}}$$

In this formula d = diameter of rope in inches.
W = weight of load and skip in short tons.
F_t = safe working stress in short tons.
N = number of thousands of feet.

It had been checked only for a "six-nineteen" rope, and the constants might require alteration for other classes.

TABLE 21.

COMPARISON OF FACTORS OF SAFETY AND OF SECTIONAL AREA OF WIRES OF THEORETICAL TAPER ROPE AND ROPE USED BY MR. BEHR IN CASE IX.

	At Capping.		1,800 ft. up.		4,600 ft. up.		4,600 ft. up.		5,994 ft. up.	
	Wire area in sq. in.	Factor of Safety	Wire area in sq. in.	Factor of Safety	Wire area in sq. in.	Factor of Safety	Wire area in sq. in	Factor of Safety	Wire area in sq. in	Factor of Safety
Theoretical Taper Rope	·475	7	·5828	7	·683	7	·8015	7	·936	7
Mr. Behr's Sectionally Tapered Rope	·595	8·8	·71	8·35	·845	8·32	1·01	8·4	1·01	7

(*Page 341*). Mr. H. H. Webb, *in the course of his remarks dealt as under with the subject of taper ropes*:—

He thought that Mr. Jennings' remark on page 133,

> "The method of construction of the ropes at Przibram is complicated, and must be costly,"

and Mr. Robeson's assertion on page 162 about the electric couple formed at each brazed joint by the acid water of the Witwatersrand mines lowering the factor of safety, called for some notice.

Mr. Catlin, who had recently visited Przibram and investigated the subject, had assured him that the manufacture of these ropes was not complicated, and that as a matter of fact he had no idea that it was so simple and easy.

Mr. Behr and he (Mr. Webb) had also visited the works of Messrs. Roebling and Sons in America, and of Messrs. Felten & Guilleaume on the Continent, and had talked the matter over with these firms, and they did not consider the manufacture of taper ropes complicated. Further, Mr. Behr would be able to give some interesting information on this subject from a very recently published work by Oberbergrath Professor Hrabák of the School of Mines at Przibram. The ores of Przibram contained a variety of metallic sulphides, and the waters of these mines should be more acid than the water encountered in the mines of the Rand. No mention, however, was made by Professor Hrabák of any such action and effect as asserted by Mr. Robeson. On the Rand, there was probably not a rope of any considerable length used which was not brazed in several places, yet the argument raised by Mr. Robeson had never been brought forward there before. Certainly anyone acquainted with mining on the Comstock in the seventies and eighties would remember the great acidity of the water in those mines, especially as regards the effect it had on pump valves, and as a matter of history pertaining to taper ropes in use on the Comstock in those early days, he would introduce here a copy of a letter from the Superintendent of the Crown Point, and also the Savage Mine, to Messrs. J. A. Roebling's Sons Co.

[copy.]

"Superintendent's Office,
"Savage Mining Company,
"Virginia, Nevada, Feb. 7th, 1900.

"Messrs. J. A. Roebling's Sons Co.,
"San Francisco, California.

"Gentlemen,—

"Replying to your enquiry concerning taper ropes for the Savage Mining Company:

"I find that in March, 1875, we received from J. A. Roebling's Sons Co. 4,000 ft. C.S. Round Taper Rope, 2 in. by 1¾ in., which was shortly after placed in vertical shaft of 1,100 ft. and incline of 1,000 ft. for a few months only—probably nine—and taken out on account of great expense in this instance.

"The ropes were then placed on reels and remained at the works, idle, for want of opportunity to use them. For what time they worked, we were well satisfied with them, and they were laid aside for no fault of the ropes.

"We sold a portion of these ropes to the Crown Point Mine, report of which is annexed.

"In the fore part of the year 1890, the Savage Mining Company sold 2,000 ft. of one of these taper ropes to the Crown Point Mine, which was worked for about three years constantly under the following conditions:—In a vertical shaft of 1,100 ft., and incline of 35° of 600 ft. from the foot of the vertical shaft.

"It gave undoubted satisfaction in every respect, and is now on reel at the works in first-class condition showing but little signs of wear from the work it has done, and is only laid by on account of there being no work for it to do.

"Yours truly,
"(Signed) W. H. PORHAM, Supt.
"Crown Point G. & S. M. Co.
"Savage S. M. Co."

(*Page 356*). Mr. H. C. Behr *replied to the various matters discussed dealing with wire ropes, as under*:—

The rope of uniform diameter, required with driving sheaves, became heavier much more rapidly as the depth increased, than a taper rope of the same strength, particularly if the factors of safety, which had so far been used with driving sheaves, were not diminished. For the same maximum stress and amount of its fluctuations, the moment of inertia of the sheaves should therefore be actually greater than required for drum winders with taper ropes, in order to secure the same smoothness of running. In the following table were given the weight of ropes of uniform section, and of taper ropes for a depth of 5,500 ft. with factors of safety ranging from 6 to 10 for a material of 245,000 lb. ultimate strength. The useful load was taken at about 8,000 lb., the skip at 5,000 lb., the conditions being the same as those of Mr. Jennings' case.

TABLE 22.

FACTORS OF SAFETY FOR TAPER ROPES AND ROPES OF UNIFORM SECTION.

		Factor of Safety.				
		6	7	8	9	10
Depth in ft.		5,500	5,500	5,500	5,500	5,500
Weight of Load, lb.		8,000	8,000	8,000	8,000	8,000
Weight of Skip, lb.		5,000	5,000	5,000	5,000	5,000
Static Load Stress		40,833	35,000	30,625	27,222	24,500
Breaking Stress of wire, lb. per sq. in.		245,000	245,000	245,000	245,000	245,000
Rope in three sections of varying diam. 2,000 ft., 2,000 ft. and 1,500 ft. long; without tail-rope.	Weight of rope, lb. ...	10,566	13,358	16,624	20,440	24,956
	Largest diam. of rope, in.	1·201	1·372	1·555	1·7521	1·9679
	Smallest diam. of rope, in.	0·994	1·096	1·1984	1·3	1·4034
	Load Stress when skip is at top of shaft ...	32,829	27,027	22,628	19,222	16,501
Rope of uniform section with tail-rope.	Weight of Rope, lb. ...	15,180	22,000	33,154	54,766	114,400
	Diam. of rope, in. ...	1·315	1·58	1·94	2·494	3·6056
	Load Stress when skip is at top of shaft ...	40,833	35,000	30,625	27,222	24,500
Factors of Safety for Taper Ropes when skip is at surface		7·46	9·00	10·82	12·74	14·84

For a factor of 7, the total weight supported by the rope at starting was nearly 1⅓ times as much with the rope of uniform section as with the taper rope for the depth given, and the total moment of inertia should be greater in the same proportion for the tail-rope, if the same steadiness of running were required.

With taper ropes, the factor of safety could be increased if needed, without thereby greatly increasing the width of drum.

* * * *

(*Page 391*). According to what Professor Undeutsch had shown in the paper referred to by Mr. Jennings, and according to the similar investigations made by Mr. J. A. Vaughan, the results of which he gave in his contribution to the discussion, and also according to general experience, it appeared that the security of a rope against suddenly applied loads or shocks decreased very rapidly with the distance between the load and the drum, starting from a certain point where the security was greatest. It was therefore wise to give a considerable length of rope between the pithead sheave and the drum, an arrangement which likewise had the effect of diminishing the angle of lateral deviation.

(*Page 404*). Mr. E. T. Sederholm's formula for the bending stress " S " in the wires of a rope, given in his contribution on page 104 and in Plate XXVI, was nothing but the old Reuleaux formula applied to a rope with 6 strands of 19 wires each, and the very simple combination to obtain the admissible load " L " consisted only in multiplying the sum of the stresses due to bending and direct load by the useful area of the wire in the rope. Mr. Sederholm's formula was also affected by the same error as that into which Professor Reuleaux fell in basing his stress calculations on the direct modulus of elasticity of the material, which gave a value too high for all cases, except perhaps that of the wires in an old flat rope. Professor Bach, in his work *Die Maschinen Elemente*, had shown this supposition to be wrong, and, as the results of experiments, gave figures for the bending stress in the wires of new unused ropes, which were only 36% of those which were calculated from the Reuleaux formula.

The reduction given by Professor Bach was, however, only suitable for new ropes of the ordinary construction, as shown by Professor Hrabák in his recent treatise on wire ropes, entitled *Die Drahtseile*. According to the very extensive experiments instituted by Hrabák, the bending stress in an

ordinary hoisting rope, like those with 6 strands of 19 wires, would be on an average about double that occurring in a new rope, or about 72% of the modulus of the elasticity "E" of the material. This agreed practically with the results for E= 21,500,000 obtained experimentally by Mr. A. M. Robeson and given in his contribution to the discussion when treating of differential stresses. For a rope of 6 strands with 7 wires the modulus, according to Hrabák, would be higher, so that the bending stress would be also proportionally higher for the same size of wire with a 7-wire strand, than with 19-wire strand in the rope. According to experiments referred to by Professor Hrabák, Lang's lay ropes retain a lower modulus of elasticity than ordinary lay ropes. They must, however, be kept from untwisting.

An interesting point brought out by Hrabák through his experiments, though this was not in the nature of a reply to any criticisms, was the fact of the rapid increase of the modulus of elasticity during the first few weeks' use of a new rope, and its more and more gradual increase until an apparent maximum was reached, after which the modulus often seemed even to decrease. This latter effect, however, Habrák pointed out, was only apparent, it being, in fact, a sure sign of the beginning of the giving way of the rope when this was observed. Hrabák remarked that the rope should then be discarded as unsafe. Hrabák recommended frequent tests of the modulus of elasticity, made in a similar manner to those executed here by Mr. Robeson, as an excellent means of inspection and indication of the condition of the rope.

Referring to bending stresses in ropes wound on drums, Mr. Jennings remarked: "At least half the total amount of rope was always under a bending strain."

Mr. Laschinger had shown that, owing to the elasticity of the drum-shell, the stress on the rope must necessarily be relieved to a considerable extent. This had also been shown in practice by some drums which had carried so many coils of rope under strain that, if the strain had remained of the full value in all the coils wound on, the drum shell would inevitably have been crushed. But assuming, for the sake of argument, that the stress did remain in the rope, was it not a well-known physical fact that the more frequently a given stress was applied and removed the sooner the material was fatigued? Evidently the Whiting hoist caused more repetitions of stress than any other winding device, including two reversals and two 70% reversals, as the diagrams given by Mr. Laschinger in his contribution showed.

One contributor, Mr. Kenneth Austin, seriously made, in substance, the extraordinary statement, that in running over the sheaves of a Whiting hoist, the rope had not time to become set or damaged. According to well-established laws, the stress was proportional, within the elastic limit, to the deformation, and the rope did certainly receive the same deformation in being wound around a sheave as around a drum, only in the Whiting it received so many more deformations, that its life must be considerably shortened thereby. As a logical weakness of Mr. Austin's remark might be pointed out the fact, that the sheaves did stand still for quite a large portion of the time, and that during such stoppage, the rope would have ample opportunity to assume the stress, which, according to Mr. Austin, it had no time to pick up when hurrying around the periphery of the sheave.

It seemed to him that Mr. Robeson's remark on page 155:—

> "What did it matter if there were differential and bending stresses there, if they did not cost anything?"

did not apply to the question at issue, that of ropes with a factor of 6 or 7 for deep shafts used with Whiting hoists. The Kimberley shaft, to which Mr. Robeson pointed in connection with the above remark, was only 1,849 ft. deep when the ropes to which he referred were used. The shafts on which the discussion hinged were to be 5,500 ft. deep. Again, the factor of safety was high at Kimberley, the writer being informed by Mr. P. A. Robbins, the Resident Consulting Mechanical and Electrical Engineer of the De Beers Company, that the factor of safety in use was 10. Mr. Robeson proposed to use a factor of 6 for the Whiting hoists he intended to instal for the shafts 5,500 ft. deep, since it was evidently impracticable to use a factor of 10, or even 8, for such depth, with a rope of uniform section.

(*Page 407*). On page 133 Mr. Jennings stated:—

"The method of construction of the ropes (taper) at Przibram was complicated, and must be costly."

But he did not show any reason for his contention. The following description of taper ropes and their manufacture, was translated from Professor J. Hrabák's new book on wire ropes, entitled *Die Drahtseile* (pp. 43, 44, and 45), from which it could be judged whether such manufacture was complicated or not:—

"When hoisting is done from very deep shafts it is necessary to use tapered ropes in order to avoid excessive weight in the rope itself. Long suspended prisms of equal strength throughout follow in profile the curves of a cubic parabola. It would be a vain attempt to reproduce the cubic parabola profile in constructing a taper rope. In point of fact taper ropes, to answer all practical and necessary requirements, are made in this way:—The lowest section is made uniform and very long (*e.g.*, 300 to 400 m.), with a certain number of wires of the proper thickness. The upper portion of the tapered rope is made of uniform portions of greater diameter, all portions being of equal lengths, say, 200 m. each.*

"In practice the section of taper ropes is varied in two ways:—

"1st. By decreasing the number of wires while retaining the same diameter of wire.

"2nd. By decreasing the diameter of the wires while retaining the same number of wires.

"These changes must be effected in the strands which go to make up the finished rope.

"It serves all purposes better to adopt the second method of construction in preference to the first method, because when a strand is laid it is impossible to alter the machine to lay, for example, an 8 wire, a 7 wire, and a 6 wire strand, equally well; the same spinning head which handles 8 wires would then have to handle 7 wires and then 6 wires, and the spinning would not be uniform. Further, when these strands are spun to form the rope there will be further irregularities. In any case the manufacturer of a taper rope by decreasing the number of wires depends somewhat on circumstances. Only certain systems of rope construction, and these not of the best, are adapted to this method of producing taper ropes.

"The second method of construction (by maintaining the same number of wires and decreasing their diameter) is much more practical and convenient.

"In the details of their construction and in actual practice such ropes are perfectly reliable, and no uncertainty need be feared if the rope-laying machinery is properly designed and the installation in which the ropes are to be used provided with drums and sheaves of the correct diameters.* Furthermore, this method is suited to any style of rope construction as regards the lay or the arrangement of the wires in the cross-section.

"The manufacture of taper ropes according to this method does not differ in practice from the manufacture of ropes of uniform section. In the ordinary process of manufacture of uniform strands the machine must be stopped whenever a spool of wire gives out and another full spool is substituted. In such a case the two wires are joined by flatting their ends, laying on top of each other and binding them together with thin copper or brass wire. The joint is then brazed and finished round. This done the wire is stretched to the proper tension and the machine again started.

"In a similar manner, the smaller wires are brazed on to the larger ones when making strands for taper ropes, only these operations are made to occur at regular intervals as soon as the required

* In order to obtain an equal strength in the uppermost cross-section (the so-called most dangerous cross-section) it would in theory be necessary to make the uniform portions of the rope shorter in length as the upper end is approached, but for practical reasons, as shown in other parts of this book, this is not done.

* In the Lead and Silver Mines at Przibram, taper ropes constructed as above have been in use for the last quarter of a century, hoisting originally from two shafts each 1,000 m. deep, and at present from five shafts, which now considerably exceed 1,000 m. in depth. These ropes are constructed of cast steel wire with an ultimate strength of 180 kg. per sq. mm. A large and varied experience with these ropes has proved that this method of manufacture is most highly satisfactory.

length of strand of uniform size (which is necessarily somewhat greater than the length of the section of rope to be made therefrom) has been completed, when the brazing on of the smaller wires begins)*

"The brazed joints in the wires where the diameter changes are distributed over a length of from 10 to 20 m. of the strand, depending upon the number of wires.

"In this manner the reduction of diameter of the rope is brought about while the strands are being laid up. As a rule, the change in diameter of the wire is according to wire gauge one or two sizes smaller than the previous one.

"The laying up of the strands into a rope is exactly the same for taper as for ropes of uniform section."

* * * *

Mr. Robeson remarked that:—

"With the acid water of the Witwatersrand mines, the electric couple formed at each brazed point would soon lower the factor below 7, and if wires were 'dropped' at intervals, as some makers proposed, the rope would be no better than a spliced rope."

Mr. Webb had given an instance of a taper rope with brazed wires, which lasted for a long time in mines noted for their acid waters. Probably the thorough oiling of the rope prevented the action of the acid.

He could not see that the "dropping" of the wires would cause the rope to be the same as a spliced rope, because in the taper rope thus constructed, the wires giving the full cross-section required for the factor of safety adopted, at the point where wires are dropped, ran clear through from the upper end of the rope (not perhaps without such brazings as may be needed in view of limit of wire length commercially obtainable). In a spliced rope, on the other hand, there was a break in a wire where it should properly be continuous.

The following table gave the construction of the taper ropes for the winders of the Ronchamp Collieries, mentioned in the early part of this reply, and designed to operate at a depth of 1,000 m. (3,280 ft.).

* The measuring of the length of strands is done during the laying up, by means of a counter driven by the machine through a light friction wheel exactly 1 m. circumference.

The ropes were constructed according to the method of dropping wires. (From *Zeitschrift des Vereins Deutscher Ingenieure*, article by A. Deeg on Twin Tandem Compound Winders, 19th July, 1902):—

Length of section m.	Diameter of section mm.	Number of wires of 2·85 mm. diameter.	Weight in kgs. per m.	Weight of section in kgs.
235	45	80	4,585	1,077
235	48	88	5,043	1,185
235	51	96	5,500	1,292
235	54	104	5,950	1,398
246·5	58	112	6,420	1,582
Total length	1,186·5 m.		Total weight	6 534 kgs

The rope was constructed by Adolph Stein of Mülhausen in Alsace. An illustration of the winder with which the above rope was used was shown by Mr. Laschinger in his contribution.

The above data and information should answer Mr. Jennings' further remark in his contribution:—

"This being so, would not the place of junction of two ropes of uniform section be a 'weak link in the chain,' and would it not be logical to insist on a higher factor of safety in the taper rope system than in that of the rope of uniform section? At present, did the Mining Regulations permit ropes of uniform section to be spliced, and, if not, how could Mr. Behr legally join a length of rope of small section on to another length of larger section?"

The reason why taper ropes had not been used to any greater extent was evidently because, with the materials of to-day, a taper rope would not be needed until a depth of, say, about 3,000 ft. had been exceeded, which at present had happened in only a few instances. The taper ropes on the Comstock were introduced at a date when rope materials had a much lower ultimate strength than those which could be had now. It was while these ropes were still in use that an important improvement was made in the strength of rope wires, and there was therefore no sufficient reason to adopt taper ropes for the depths then worked.

Mr. Jennings had quoted Professor Undeutsch repeatedly in his contribution in order to demonstrate the inadvisability of using taper ropes. (He Mr. Behr) could find nothing in the Professor's treatise indicating that taper ropes would be more unsafe than those of uniform section. It had been shown by several contributors, and also by him in his reply to the London discussion, that the taper rope constructed in the long terminal length of uniform section, as recommended by Hrabák, or by Felten and Guilleaume, of Mülheim on Rhine, had a much higher factor at the lower end than at the upper. It was evident, therefore, that the lower end of the taper rope was safer than the rope of uniform section with tail-rope and equal factor of safety. The taper ropes for the cylindro-conical drums, herein discussed, were designed with a very long lower section, and had therefore a high factor of safety at the capping. The rope thus had its greatest strength where shocks, as Professor Undeutsch and Mr. J. A. Vaughan pointed out, were most dangerous, *i.e.*, when the load was near the surface. On the other hand, the hoisting rope of uniform section proportioned for a factor of 7, when operated with a tail-rope, had that same factor when the load was at the surface just before being discharged and had thus a very much lower strength than the taper rope at the very time when strength was most needed.

Mr. Jennings referred to a formula given in Kent's Mechanical Engineer's Pocket Book as applicable to taper ropes. The formula given by Kent was not suitable for taper ropes. It is the well-known formula for a suspended body of equal strength for statical conditions alone. It was also well known that the true form of this was not a taper. It would be absurd to try to construct a tapering rope according to the formula for equal strength referred to, because in a tapering rope the lower smaller part should, as pointed out in the preceding paragraph, have a greater statical strength than the upper part in order to resist shocks.

He had referred in the early part of his paper to two large mines where taper ropes were in use, or were about to be used, viz., the Ronchamp Collieries in France, and the Preussen shaft No. II in Westphalia. These were recent installations. At Przibram there were five shafts at which taper ropes were used, two of these over 4,000 ft. deep. The Ronna Mine at Kladno, Bohemia, had a shaft only 1,456 ft. deep, where, according to Hrabák, taper ropes were used. Léon Poussigue, in the *Bulletin de la Société de l'Industrie Minérale*, mentioned the Hugo Mine in Westphalia, with a shaft 1,944 ft. deep, as using taper ropes. At the two last-named places, however, he failed to see why taper ropes were employed, since the depth was not sufficient to warrant their use.*

In his concluding remarks Mr. Behr said: Taper ropes were as reliable as those of uniform section, and would cost less for deep shafts than ropes of uniform section of equal effective strength. Taper ropes, made as they should be with a long lower portion of uniform section, had a greater margin of safety at the most dangerous section than ropes of uniform section with tail-ropes for the same factor of safety.

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* According to a communication received by Mr. Behr, since making his reply, the "St. Egydyer Eisen-und-Stahl-Industrie-Gesellschaft" of Vienna, has manufactured quite a number of taper ropes; for example they have furnished three taper ropes for the Theresia Shaft of the Witkowitz Coal Mine in Mährisch, Ostrau. Particulars as follows:—

Lang's lay rope, ultimate strength of material 180 kgs. sq. mm. (256,000 lb. sq. in.), 108 wires.

	Length.	Diameter wire	Diameter rope.
Upper section	400 m.	2·2 mm.	34 mm.
Lower section	450 m.	2·4 mm.	37 mm.

A rope for Giovanni Peron in Schio, a taper rope 570 m. in length in four sections as follows: Construction (6+1) (6+1). Six strands around one, each strand 6 wires around 1.

	Length.	Diameter wire.	Ultimate strength of material.
Upper section	142 m.	2·5 mm.	120 kgs. sq. mm.
Upper section	142 m.	2·2 mm.	120 kgs. sq. mm.
Upper section	142 m.	2·0 mm.	120 kgs. sq. mm.
Lowest section	144 m.	1·8 mm.	150 kgs. sq. mm.
Total	570 m.		

Another rope for Giovanni Peron 1,400 m. in length (4,600 ft.). Construction same as above, in two sections.

	Length.	Diameter rope.	Diameter wire.	Strength.
Upper section	500 m.	25 mm.	2·5 mm.	120—130
Lower section	900 m.	20 mm.	2·2 mm.	120—130

They have also supplied taper ropes for boring in Galicia. Construction, 6 strands around a hemp core, each strand composed of 15 wires, around 9 wires around a hemp core. Strength of material 120—130. Total length 600 m.

	Length.	Diameter rope.	Diameter wires.
Upper section	200 m.	22 mm.	1·2 mm.
Lower section	400 m.	18 mm.	1·0 mm.

From the "Journal of the Mechanical Engineers' Association of the Witwatersrand, Vol. III, No. 5, 1904:—

NOTES ON THE CORROSION OF WIRE ROPES BY MINE WATER.

Paper by Mr. W. S. THOMAS (*Associate Member*).

The subject of corrosion in wire ropes due to acid water in the mines is one deserving the atention of the engineering profession, and has lately been brought into prominence. The failure of many ropes recently may be attributed to this cause, and apparently each year we have a greater number. The subject is one of which we know but little at present, and we are constantly adding to our stock of knowledge in this respect.

At the request of our President, and through the kindness of Mr. Webber, General Manager of the Rand Mines, the writer is allowed to give the details of some experiments recently made in the water from the Crown Reef Mine.

This water, together with the water from the Bonanza, is pumped by the Crown Deep through their shafts. The water of the Crown Reef probably contains the greatest amount of acid of any water on the Rand, and therefore the experiments given below may be taken as the maximum action of mine water in any given time. The Bonanza water also contains a large amount of acid, but not nearly so much as the Crown Reef water. In pumping the water great difficulties are experienced with the plungers in the pumps and the pipe columns. These pipes have to be lead-lined, but even that does not entirely overcome the trouble. The greatest possible care is used to keep this water from getting on the ropes, and the examination for corrosion is very thorough, this being necessary owing to the very acid water being pumped through the shaft.

The water from the Crown Reef enters the Crown Deep in a small cascade of about 8 ft. in height, and the experiments were made by hanging wires by means of string in the water about 3 ft. from the top of the fall. The wires, which were hung in pairs, were of crucible steel ·061 in. diameter, and of galvanised plough steel ·063 and ·066 in. diameter. These steels are both what might be considered high carbon steels, but unfortunately the writer is unable to give any idea as to the amount of carbon contained in either. A high carbon steel is generally supposed to be affected by acid much more rapidly than a low carbon steel or iron wire. Personally, the writer has never made any experiments to prove this, and has in one or two instances noted quite a contrary effect. An instance of this can be seen in the wires of the sample marked "Z." The outer wires are 90-ton crucible steel, and the core wire is either of soft iron or mild steel, which would be lower in carbon. The inside core wire is found to be very much more affected, though the acid was probably acting the same time on each. There was a set of experiments made by the Admiralty lasting over a period of some twelve years, and they found high carbon steel more affected than low carbon steel or iron by sea water. Amongst the wires shown here there is very little difference between the amount of corrosion which has taken place on the crucible and plough steel, though it is safe to assume that the plough contains the greater amount of carbon. The wires, it will be noted, are not eaten away to quite the same amount throughout their entire length. Just near to where the string was tied in every case a greater amount of corrosion has taken place, and this has also occurred where the wire has been bent. This might be accounted for in the first instance by the fact that the hemp string holds the water containing the acid and gives it a greater chance to act. This would lead us to the assumption that untarred hemp cores in the centre of strands would aid corrosion in ropes; with tarred hemp cores, however, there should be no chance of the water being held, although in sample "Z" above-mentioned the wires are corroded where they have been in contact with the hemp core of the rope. This core was presumably tarred when the rope was made. The bends in the wire would also tend to hold the water at that particular place, and would probably account for the greater corrosion noted at such places. Also each wire is very sharply pointed at the ends, and this appears to be always the case where strong acid is acting. Another thing which should be noted in regard to these samples is that the acid action on the crucible steel is not shown by pitting to any great extent, though on the plough steel it is thus shown

in the earlier stages. That plough steel pits more than crucible in this water was pointed out to the writer some time ago by Mr. Wilson, Resident Engineer of the Simmer and Jack, and it is certainly so in this instance.

The wires which were immersed in Crown Reef water are exhibited here to-night, and are as follows:—

SAMPLE No. 1 A.—These two pieces are crucible steel, and were removed after having been immersed for a period of 24 hours. They are to be found eaten away to about ·057 in. diameter in the larger part, and about ·020 in. diameter in the part nearest the string.

SAMPLE No. 1 B.—These two pieces are of galvanised plough steel wire, and were also removed after 24 hours. In the larger sample there are places where the wire is still the original size, and this is also the case with the smaller, though in the latter there are distinct signs of pitting. The other ends of the wires are eaten away to about ·045 in. diameter. This seems to show that the galvanising of wires may resist the action of the acid, and thereby preserve the ropes to a certain extent.

SAMPLE No. 2 A.—These are two of the crucible wires after 36 hours immersion. These have been eaten away to about ·019 as the smallest diameter, and ·055 as the largest.

SAMPLE No. 2 B.—Two of the galvanised plough steel wires after 36 hours. The smallest diameter here is about ·024 and the largest about ·057.

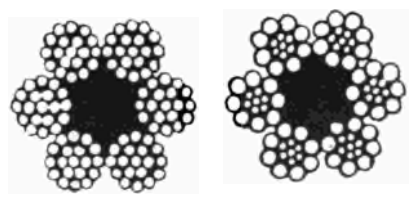
SAMPLE No. 3 A.—One of the crucible steel wires taken out after 38½ hours. This was found to be eaten entirely through near where the string was tied. The largest diameter is about ·054.

SAMPLE No. 4 A.—One of the crucible steel wires, after being in the water 37 hours, was found to be on the point of breaking, and was removed earlier than was originally intended. The largest size here is about ·052.

SAMPLE No. 3 B.—Two of the galvanised plough steel wires after being 38½ hours in the water. The average diameter is about ·025 in the one wire and ·028 in the other. The smallest diameter is like a pin point and is almost on the point of breaking.

From the looks of the samples shown here, it would be safe to assume that a ·061 wire would only last from 37 to 40 hours in this water. Should water containing this amount of acid get on a rope, it would be most dangerous. Fortunately few of the mine waters would have as rapid an action as this of the Crown Reef.

A peculiarity of the acid action in the mines here may be noted, and is that after the wire which has been affected on the inside of the rope has been cleaned up, it often has the appearance of having been badly worn through chafing on the inside core wires, or what might be termed internal friction. A sample marked "X" is shown of wires taken from a compound crucible steel rope which was badly corroded inside. When the rope was opened out, after having been taken off, the inside of the strands was filled with a red oxide, and there was no doubt as to corrosion having been the cause of this apparent wear. Since these wires have been taken from the rope, they have been thoroughly cleaned, and you will note that there are no signs of pitting, the appearance of the wires being that of wear only. One of the worst features of the action of acid water here is that it affects the inside of the strands of a rope and is often never visible from the outside, except that it is shown in a certain way in one or two constructions. Generally speaking, a rope in which the strands are composed of wires wrapped around a core of several wires (this in mining ropes would be either what is known as the compound construction or the 19 wire construction) inside corrosion can be readily detected.



Sections 6/19 and Compound Constructions of Rope.

The evidences of internal corrosion in the above construction are shown on the outside of the rope by the fact that the wires do not bear equally and seem to be loose in many places. The first evidences are when a knife is put between two wires on the crown of the strand, and the wires

Sample 1 B. Sample 1 A. Sample 2 A. Sample 2 B. Sample 3 B.

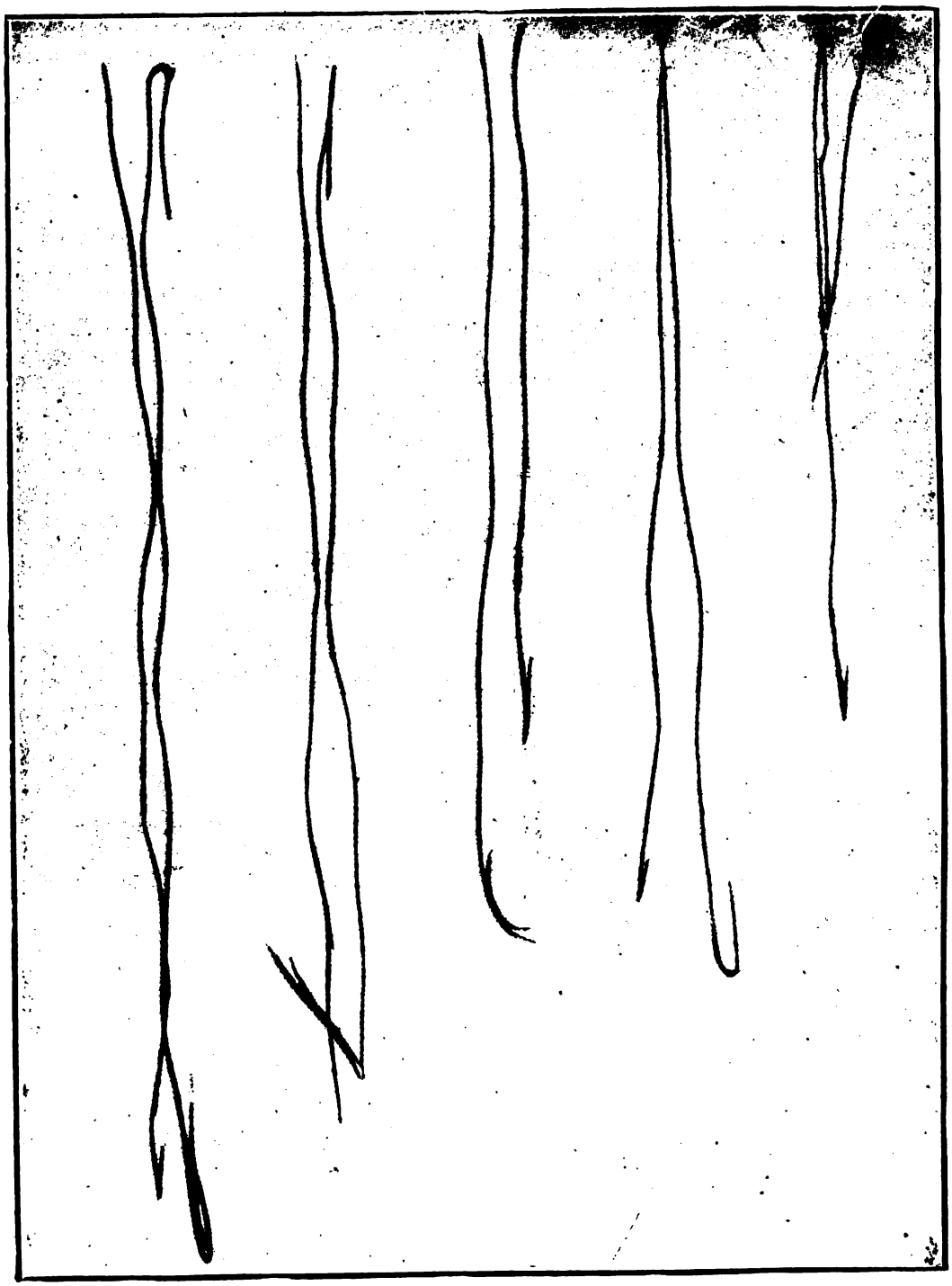
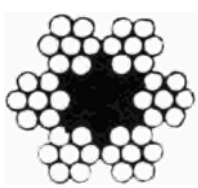


Illustration of wires after having been in the Crown Reef water for periods varying from 24 hours to 38½ hours.

can easily be moved aside. After the corrosion has gone further, the wires stick up like the centre of a carriage spring and can easily be pressed down with the finger. In this case the blade of a knife can be pressed under the back of the wire. The sample marked " Y " is shown to illustrate a rope made six strands of eight wires over seven (compound construction) which has been internally corroded, and the above signs are all plainly visible. What is dangerous about a rope corroded in this way is not so much the actual reduction of the area of steel in the section as it is the fact that nearly every strand has one or two wires which are not bearing any portion of the load, and greater strain is therefore put upon the other wires. If the wires which are bearing the greater portion of the load should break, it would suddenly throw the load on the corroded wires with a slight jerk, thereby causing the entire rope to be broken. When a rope shows these signs it is impossible to judge how many wires are bearing the load, and for this reason it should be removed at once.

Thus far the writer can give no rule by which internal corrosion can always be detected in the 6/6 and 1 construction of rope.



Section 6/6 and 1 Construction of Rope.

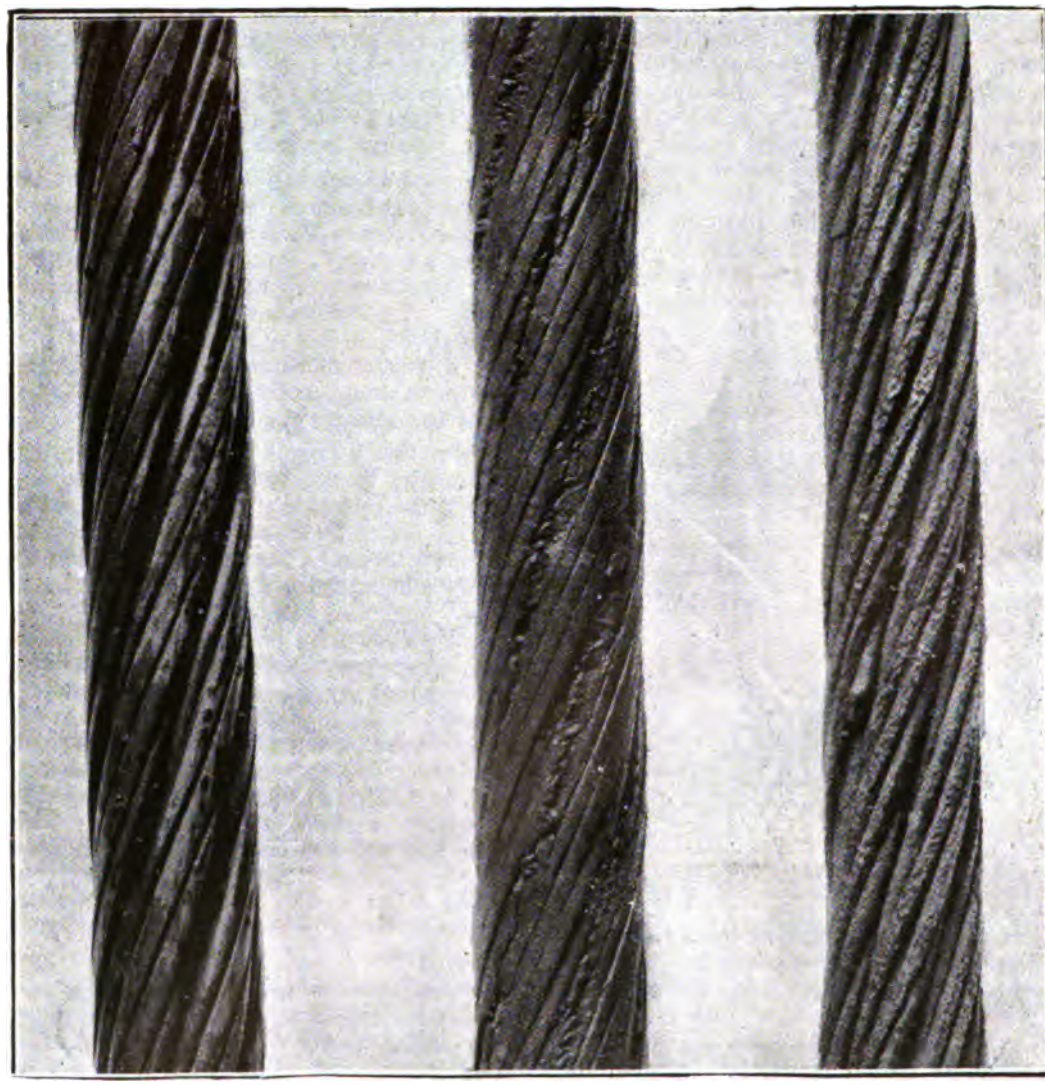
It is always accompanied by a reduction in diameter, but a reduction in diameter may be due to so many other causes that it can hardly be given as a rule for determining internal corrosion. In many cases corrosion, if taking place, is indicated by considerable pitting between the strands just at the point where the wires turn into the rope. There have been cases, however, where no pitting is visible from the outside and the rope is found to be badly corroded inside. Such a case is illustrated by the sample marked " Z." The rope iself is shown here, together with some of the wires, which have been removed from another portion. The reduction in diameter in this rope is from 1¼in. full (3⅜ins. circ.) to 1in. and in some places 31-32ins. You will note that this wire is very badly pitted on the inside and no evidence of this pitting is visible on the rope itself. Another peculiar thing here is that the corrosion has been going on for some time and has then stopped, owing, probably, to the fact that the shaft became very much drier after the rains ceased. This conclusion is arrived at on account of the pit-marks being covered over by the tar which was applied to the rope on the outside.

The sample marked " Q " is shown to illustrate corrosion as exhibited by pitting. This rope was worked in an incline shaft for about a year. Possibly the acid in the water was worse at some times than at others. The analysis of the water in one of the shafts of this mine shows ·01127 per cent. of free sulphuric acid. It is probable that the amount acting on this rope was two or three times this.

Acid action which is expressed by pitting in this way is not very dangerous on account of the fact that it can be readily seen, and the action is as much on the outside as on the inside. Steam corrosion due to the ropes being worked in an up-cast shaft, where engines are worked below, is generally shown by pitting very similar to this. A corrosive action due to steam can be prevented by galvanising the wires of the rope. As a matter of fact nearly all the corrosive actions in the collieries at home can be prevented by galvanising, which is a thing to be strongly recommended under certain circumstances. The writer has not yet completed experiments to show whether it would be a preventive in this mine water, though one or two galvanised ropes have certainly given good results here.

A most peculiar action of acid water can be seen on the flue tube of a Lancashire boiler at the Meyer & Charlton mine. The action here is the eating of a half-inch or three-quarter inch plate entirely through in isolated spots and not showing any effect on other parts as far as can be seen. In one place a piece of one of a rivet is hammered very thin and is exposed on both sides to any action of the acid, but this has not been affected. Also no pit marks are noted in any parts of the plate, except where the acid has eaten entirely, or almost entirely through, as mentioned above. Probably this peculiar action may be due to

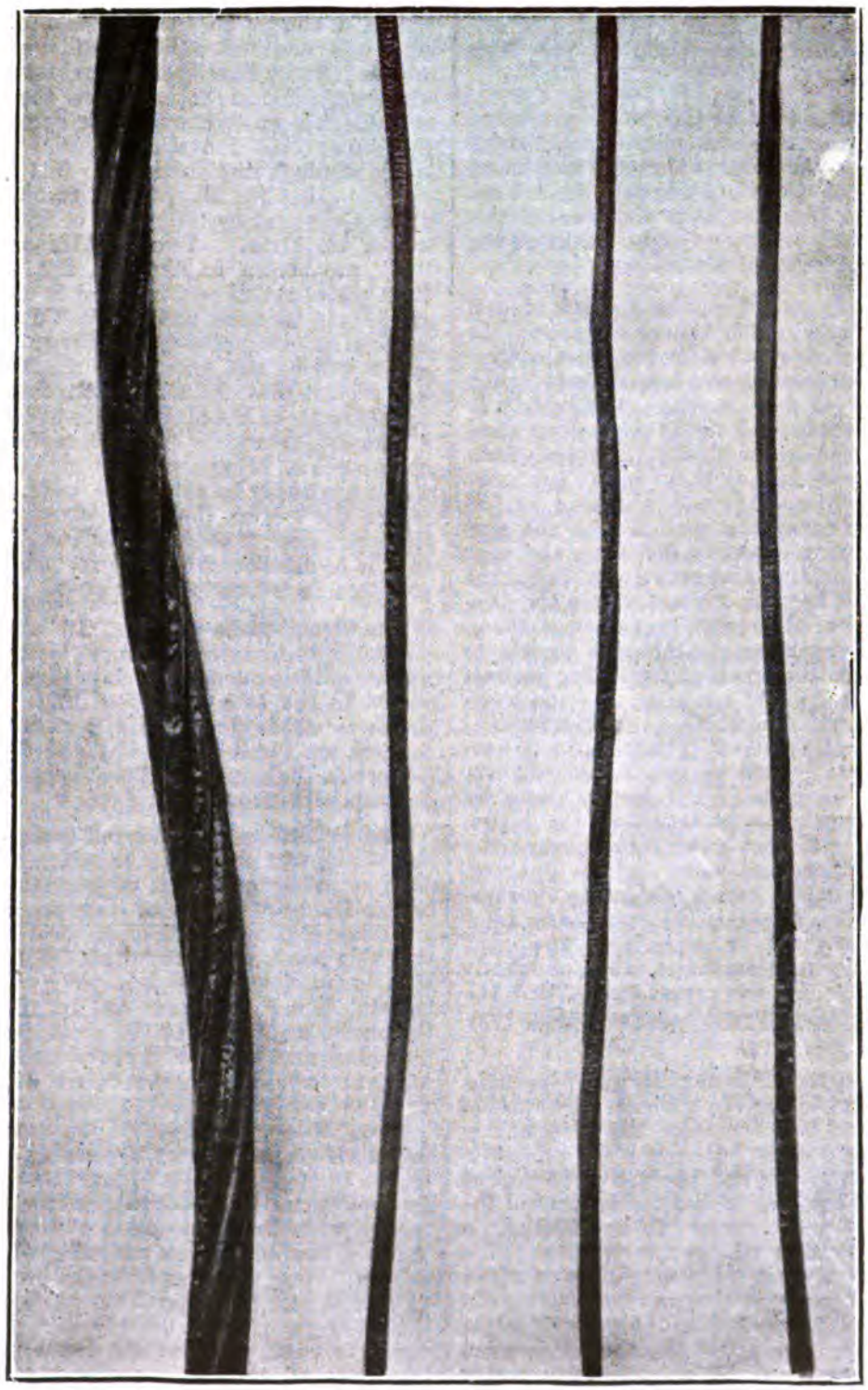
Sample Z. Sample Y. Sample Q.



Sample Z.—6/6 and 1 construction of rope. Illustrating internal corrosion without showing any signs of corrosion on the outside of rope.

Sample Y.—Compound construction of rope. Illustrating method of detecting internal corrosion in this construction. It can be seen in this illustration that the wires are sticking up in places.

Sample Q.—6/6 and 1 construction of rope. Illustrating corrosion as expressed by "pitting"



STRAND WIRES FROM SAMPLE Z.

The portion of the strand which shows corrosion was against the core on the inside of the rope. On the wires it is only on the inside where corrosion is noticeable. The middle wire is the core wire, and is either of low strain steel or iron.

the fact that the plate was not homogeneous and contained places which are very high in carbon.

The water used in this boiler was taken from a well and was pure enough under ordinary conditions. During some heavy rains water which had run over the tailings heap got into the well and caused the trouble in a matter of eight weeks as the maximum time of action.

Speaking generally, the amount of acid in the water of the various mines may be considered as greatest in the older outcrop mines and existing to a lesser degree in the first row of deep levels. Fortunately in sinking shafts and newly opened up deep levels there is practically no trouble from this source, but as these mines get more opened up, and there is a chance for the water to come into contact with the pyrites, we may expect to find the water very strongly acid. Unless we can find some method of keeping this water from the deep level ropes, or prevent its acting on them, we will experience considerable trouble in the future. At present no unfailing method of preventing acid action on wire ropes has been found, except that of keeping the water from the ropes. This has been successfully done in one or two mines and can be done in most of the incline shafts by paying particular attention to the boxes, and seeing that no water is dropping on the ropes, either from these or from the roof. This, no doubt, comes within the Province of the Mine Captain, but considering what might be saved, it would be a very good thing for you as engineers, who are directly responsible for the ropes, to see that the Mine Captain gives this subject some attention.

The original intention of this paper was merely to show the amount of corrosion which had taken place on the wires in this strong acid mine water, but the subject is a very large one and needs a great deal of study at the present time on account of the fact that acid seems to be affecting a greater number of ropes each year. The action also takes place now in some mines in which no acid has previously been observed. Before ending the paper it might be well to give a few theories which seem to be borne out by peculiar facts which have been noted. In the first place whether the action of the acid on the steel causes a change in the character of the steel is a question which is still undetermined. There have been several instances where such appears to have been the case, and others have expressed their opinion to the writer that the wire might become brittle through the action of this mine water. That the character of cast iron is entirely changed by the action of the mine waters we know. Two samples marked "I" are shown to illustrate this point. Both pieces are of cast iron and have been acted upon by mine water here. One piece is taken from a pump in the Crown Deep, and it will be seen that while the form of the original piece is fairly well preserved, the character of the material has undergone a complete change. This piece has the appearance of being a piece of pure carbon and it can easily be cut with a knife. Looking at the section through a glass, it has the appearance of ordinary cast iron, except that it is discoloured in places and the structure is not quite so compact.

The second piece marked "I" shows a portion of the iron as it was originally with a layer of the material, which has been subjected to the acid, on top of this. The depth to which the acid has penetrated is marked by a well-defined line and the difference in the structure of the material can be seen with an ordinary glass.

The writer has knowledge of some peculiar changes in steel wires in which no pitting or other evidences of corrosion are visible from the outside of the wire, but to no other explanation than an acid action can this change be attributed. Whether this is an acid action has not yet been proven, but if cast iron can be changed as above mentioned, it is quite possible that steel may be affected in the same way. Unfortunately this matter is not in such a state that it can be presented in this paper. The writer has had the opportunity of viewing some very interesting photographs of steel, but is unfortunately unable to show them here to-night. The photographs were the longitudinal and sectional views of two wires when magnified 750 diameters. One wire was from the drum-end of a rope and the other from another portion of the same rope which had been subjected to wear, vibration and corrosion. The structure of the steel was changed very materially in the photographs of the latter wire and a short description of the appear-

ance only will be attempted. Whether the change is due to the corrosion or to some other cause is at present unknown.

The report by the expert who made the examination was to the effect that the change was similar to the change which would take place if the wire had been heated red hot and suddenly cooled by plunging in water. This would seem to indicate that the change was due rather to an acid action than to vibration or wear.

The photograph of the wire taken longitudinally shows the lines fairly regular and straight. Some portions of the picture are white and some black, each representing different constituents in the steel. These are fairly evenly distributed in the good piece. On the cross section the steel seems to be homogeneous with the black and white places evenly distributed. On the photographs of the parts affected by the corrosion and wear the appearance has entirely changed. In the longitudinal view the lines instead of being straight are very wavy. The whites and blacks of the picture are massed in various places.

In the cross section there is a distinct crater showing. It is very irregular and the particles of steel seem to be all pointed toward this crater. The general effect is also wavy in the cross section.

As corrosion *per se* is a subject more in the province of the chemist than the engineer, it is hoped that if any of our chemical friends are present to-night, they will give us the benefit of their views on the subject.

What is needed is some reliable method of preventing corrosion taking place in wire ropes, and if the discussion to-night should be the means of our discovering some such method, this paper will not have been in vain.

THE PRESIDENT (Mr. A. C. WHITTOME): I think we all congratulate Mr. Thomas most heartily on the excellent paper he has presented to us. Of course, it is an open secret that these experiments were carried out with special reference to the Robinson Deep accident, but I hope none of the members will take advantage of this paper to make any particular reference to the trial in that case. There is no question that the corrosion of ropes is a subject that will have to be gone into most minutely in the near future. Personally, I have no experience of the corrosion of wire ropes, but to show that some of the outcrop mines are very much affected by the corrosive action of the mine water, I have brought a sample of a pipe which has only been in use about 30 days. The water has eaten right through it. It might be well in discussing the corrosion of ropes, therefore, to devote a little attention to the corrosion of pipe lines, because I know in this instance the cost in two years renewing pipes destroyed by the acid water has amounted to somewhere between £5,000 and £6,000. The cost in pipe lines might readily be more than the cost in ropes, and with the rising mains coming up the adjoining compartment to where the ropes are, you might easily have a jet of water from a corroded main playing on the rope whilst the latter was stationary. Mr. Thomas referred to the action of acid on cast iron. I remember one instance at home in the case of a tidal mill. The cast iron channels in which the sluice gates worked were at certain periods of the day immersed in salt water, and after about ten years the iron was entirely changed to a substance resembling carbon. You could take a penknife and scrape a hole right through it as easily as through a black lead pencil

Mr. J. R. WILLIAMS: I would like to refer to one or two points which Mr. Thomas raised to-night respecting the extra amount of carbon. I do not want to teach you elementary chemistry, but it is a well-known fact that if you have perfectly pure zinc and you attempt to dissolve it in sulphuric acid of any strength you cannot do so without great difficulty. But if you have a ½ per cent. of any impurity, such as lead or iron, it dissolves very freely and very readily. Therefore this, I think, accounts for Mr. Thomas' theory of the excess of carbon in the steel dissolving very readily. Take the case of the cast iron of which he showed us a specimen. This is, practically, pure graphite. Cast iron may contain up to 4½ per cent. of graphite and only ½ per cent. of combined carbon. If you have 4½ per cent. of graphite mixed with the iron, you can well understand how you can get an electric couple which will easily account for the difference in the solubility of the steel. With reference to the internal wear of the ropes, it it quite possible with a rope going down, say 1,000ft. that it will be wet at the bottom, but in coming through the shaft there

is a possibility of a stream of water wetting the inside and not the outside. After long experience of the Rand I notice that the water at the junction of the free milling and pyritic ores has the most corrosive action of any water I have ever known. About 1895 I was asked to investigate some water columns which were wearing out about once a month. This was at a company which does not now exist—the Pioneer. I suggested to the Manager that he should add 3 or 4 bags of lime per day to the water and he would never have any more trouble, and I know the column pipes lasted up to the war. It may seem to some people an extravagant measure to add lime to the water underground, but we should remember that every gallon of water which is obtained from the mine, and is used for mining purposes, has to have lime added to it, and why not take the bull by the horns and add the lime at once at the very bottom of your shaft? By so doing I am convinced from one or two things that the renewal of your pipe columns, and the renewal of your ropes, would not be anything like so expensive as they are at present. I simply offer the suggestion, and I hope in future discussions that you will bring the whole matter up and thrash it out thoroughly. That pipe on the table reminds me of another peculiar proposition. There is a mine in Anglesea which has been working since 1874, and I believe there has never been a miner underground, and yet they pay handsome dividends. My father put up a windmill for the purpose of pumping water. The sulphate of copper in the water corrodes the pipes, and every time they put in a new column for that pump they find more than enough copper in the old column to pay for three or four pipe lines. The theory Mr. Thomas has used with regard to the inside corrosion I think opens up quite a new era for the chemist, for the metallurgist, and principally for the engineer.

Mr. THOMAS: I would like to ask Mr. Williams whether he considers it is due to any impurities in the plough steel that it should show more effects of corrosion than the crucible steel.

Mr. WILLIAMS: I want to make myself as clear as possible. In the ordinary way, if a sample of any steel is sent to the chemist, he will analyse the percentage of iron and the percentage of carbon. Now that is quite wrong. He does not show the engineer what he requires. He should show the percentage of graphite present, that is, free carbon as against combined carbon. In Sir William Seaman's laboratory we made some very careful tests indeed, and we were particularly careful about the difference between carbon present as combined carbon and free carbon. If you have a piece of carbon present next to a piece of steel, you can well understand pitting taking place.

Mr. VAUGHAN: I think Mr. Thomas' paper is a most valuable contribution. I fancy it is a contribution that can hardly be treated adequately in anything like a topical discussion. Before I start on a discussion I should like to know the chemical analysis of that water. Until I see that analysis I should be inclined to think that it may be possible to get very nearly the same effects with pure water. I really believe that if you suspend a wire in pure water and you add the impact, that it may be possible to wear it away and eat it through by rapid rusting. That I think rather explains why the wires were worn at the places where they were tied. There would be extra friction there. There would be a continual movement of the wire. You can put a piece of wire into mine water in a bottle and leave it there for months, and even with very corrosive mine water you will get practically no effect at all, except just at the water line near the neck of the bottle where you get the air. You can put Mr. Thomas' wire in a strong acid solution, 10 per cent. say, and leave it there for a week, and it is quite possible, with even a little shaking occasionally, that it will not be eaten through. So I am rather forced to the conclusion that there is more than mere corrosion in this experiment, and I should like to see experiments conducted to try to find out whether this is the case or not. I have met in my time some peculiar effects of corrosion. I have noticed the difference of corrosion in evaporators and in boilers. I have worked boilers up to 30 degs. density of salt water, and was never troubled with corrosion there. I have worked boilers with distilled water, and provided you treat your boilers well and scrape the pits directly they begin forming, you will have very little trouble with corrosion. I have never seen corrosion having the effect in a boiler that it has in an evaporator, and I am forced to the conclusion that the excessive corrosion in an evaporator is

due to the swirling of the water—the actual wear on the plates being increased thereby. Of course, we know that it does not require actual acid water to corrode iron. Pure water will corrode iron, and it will do it more so if you can put it in motion. The photographs Mr. Thomas refers to I have seen, and there is no mistake there are some very marked changes revealed in them. It is a very strong assertion to make that steel is porous to acid solution, and these changes in the state of the steel may be due to the alteration of the arrangement of the molecules, due to rapidly inverted stresses. I look upon that as a more reasonable deduction.

Mr. J. R. WILLIAMS: Mr. Vaughan has drawn my attention to an omission I have made. It is a well-known fact that if you get absolutely pure water, and add cyanide of potassium to it and put a piece of gold leaf in it, that the cyanide of potassium will dissolve the gold. It will not do it entirely by itself, it needs something more, which is the admission of oxygen. With regard to the wearing away of these strands, I take it that these wires were not entirely under water. The water was falling over them, and under these circumstances you would get very strong oxidation there. All these things are really very simple chemical propositions, but the paper is of such immense importance that I do not think it would be wise to attempt to thrash the matter out to-night, because it deserves the careful consideration of every engineer on the Rand.

Mr. MAYHEW: I am extremely glad that a chemist of the reputation of Mr. Williams should raise the question of oxidation. It is a point which has been considerably overlooked. The test was made by putting some wires in a solution of sulphuric acid. They were never interfered with by oxidation, and that is the reason they did not eat away in the time expected. The fact that ropes working here in acid water of extremely weak solution are allowed to come to the surface and oxidise has, I think, a very great bearing on the corrosion of wires. I hope that Mr. Williams will bring this matter before his Association, so that those gentlemen can consider this great point—that why, since the war we find ropes eating away far more rapidly than before the war? We have found very many ropes corroded internally, which are supposed never to be attacked by water at all. In an upcast shaft we have the nitrous fumes of dynamite coming from these shafts, and it is a very great point if these nitrous fumes do not attack the wires internally. The question of oxidation seems to have been considerably overlooked in all enquiries that have been made generally on the question of the corrosion of ropes. I can give one practical experience of wires left standing during the war. At the point where the water had risen and fallen in the shaft, there was nothing left of certain wires (which were galvanized) except the hemp core, but below the point of oxidation the wires and the strands of the rope were still there.

Prof. PRISTER: Allow me, please, as a visitor, a few remarks on the very interesting paper of Mr. W. S. Thomas.

I quite agree with Mr. J. R. Williams that the main cause of the rapid destruction of the wire ropes is an oxidation process. The rope in moving up and down the shafts is exposed to all the necessary conditions required for starting and continuing a rapid and energetic oxidation process of the wires, viz., (1) moisture, (2) temperature, (3) air, (4) a diluted solution of iron sulphate, (5) free sulphuric acid.

In order to facilitate the understanding of the chemical process, which we will try to explain, let a drop of the acid mine water fall upon one wire.

The free sulphuric acid, if any present, is neutralised by the iron of the wire, and we have here a rich solution of ferrous sulphate.

In contact with the wire this ferrous salt becomes a basic salt, which in contact with the oxygen of the air is rapidly oxidised to a higher iron oxide. The acid and the ferrous salt of the mine water made also a small wound, if we can speak so, on the surface of the wire.

The next underlying molecule of iron of the wires comes in contact with this iron oxide lying upon it; this molecule of metallic iron reduces this oxide, that means it takes some oxygen from the overlying drop and oxidises itself to a ferrous salt. The so reduced molecule of iron oxide of the surface, exposed as it is to the moist and hot atmosphere of the shaft, takes quickly oxygen from this air, so replacing that which it has given to the underlying molecule of

iron. This re-oxidised molecule of the surface is yielding again oxygen to the underlying iron molecule which has been previously oxidised. This underlying molecule of iron oxide is now able to yield a molecule of oxygen to a now deeper lying molecule of iron.

This process is continuous, and so the oxygen of the air is transported from one molecule of the surface to the central part of the wire. We see that one single drop of the acid mine water which has attacked one point of the surface of the wire can destroy the wire, without the aid of more acid water, wanting only oxygen and moisture existing in the atmosphere of the shaft.

The penetration and progress of the oxidising process is further facilitated by the spongy condition in which the surface of the wire is transformed by the process itself. First of all the volume of the wire is increased, as a molecule of steel, when oxidised to a ferrous or ferric salt, occupies a larger volume by the increased matter (iron plus oxygen). As a consequence of the increased volume, the surface becomes puffy, and so a larger surface exposed to the action of the air. The other elements of the steel which are not attacked by the oxygen, viz., the carbon existing there in a graphitic state, are gradually raised to the surface and expelled, so leaving a hole, and also increasing the surface of the wire exposed to the oxidation of the air.

When a drop of this acid mine water is falling upon one of the external wires forming the rope, the oxidation process as described is disturbed by the friction of the rope when passing the different wheels, winding drums, etc. Here the wire is undergoing a kind of cleaning process, the oxidised surface is rubbed off, and so the oxidising process is retarded and prevented from penetrating to the central part of the wire. The internal wires of the rope on the contrary are not exposed to such a friction, the surface of the wire is not cleaned from time to time, therefore the oxidation process when once started on the surface can go on undisturbed till the rope is destroyed.

This explains, I think, why Mr. Thomas found the internal wires more corroded than the externals. The externals were mostly affected by the corrosive action of the acid water, only a slow dissolving process, when on the contrary the internal wires were exposed to the corrosive action of the water, plus the undisturbed oxidising process, leaving for the moment unconsidered the better protection that the external wires may have had from the tarring of the rope.

One of the foregoing speakers called your attention to the influence that the bad fumes of the dynamite used in the mine may have upon the rope.

I quite agree wtih him that in reality they may start and facilitate this oxidation process under discussion. Not all the dynamite employed in the mine is exploded regularly to carbonic acid, water and nitrogen; a part of it by some cause or other may only burn, and consequently produce a large quantity of bad fumes, containing oxides of nitrogen. The largest part of those bad fumes is retained in the mine by its moisture, but a fraction is certainly brought up the shaft, mixed with the mine atmosphere. The oxides of nitrogen are very active oxidisers, and the smallest quantity is sufficient for helping to initiate the destructive oxidation process we described.

We see from this that for preserving the wire ropes from corrosion we must fight principally two enemies: (1) the oxidation process, and (2) the dissolving action of the free acid that may exist in the mine water.

In order to prevent the starting of the oxidation process we must cover the wire ropes with something resisting to the action of the oxygen, moisture and temperature existing in the shaft. From the metals at our disposal the zinc is the cheapest.

Galvanized iron ropes will certainly last longer than ordinary steel ropes. It is evident that this thin zinc sheet we put on iron by galvanization cannot last for eternity, and cannot stand the attacks of the free acid of the mine water.

As soon as the protective cover is destroyed on a point of the wire by a blow, a kick, or other mechanical causes, the oxidation process will start on this point and destroy the wire rapidly.

The oxidation process on a galvanized wire will proceed, when once started, much quicker than on an ungalvanized wire, in consequence of the galvanic couple which is here actively formed by the covering zinc and the underlying iron.

Let us have a look on one point of a galvanized wire which has been wounded by a blow, and so the protective zinc exposed.

The above described oxidation process will soon start on this point and will be accelerated as we said by the galvanic action of the couple formed by the zinc covering, the rest of the wire lying in contact with the neighbouring healthy iron molecules. We know that zinc and iron are forming a very strong electrical couple when plunged in diluted sulphuric acid, or in a solution of ferrous salt, or in simple contact with moist iron oxides, as we have it here.

The oxidation process started on one point of the surface will continue under the protective zinc cover as the oxygen is transferred from one iron molecule to the other as described. The volume of the wire increases in consequence of the absorbed oxygen, the zinc cover is raised and probably will fall away, so exposing a larger part of the wire unprotected to the attacks of the mine water and air.

Around the wounded wire gradually a ring of oxidised iron is formed and the wire will break clean off by bending, as only the central part of the wire may be healthy.

This process explains also why one of the speakers found a rope hanging in a shaft during the war relatively preserved in its parts plunged in the mine water, when on the contrary the part hanging free in the shaft was much more corroded.

We have seen that the mine water contains a large quantity of ferrous sulphate. This salt is very avidious of oxygen. Every molecule of oxygen that may have been dissolved in the mine water is fixed by this ferrous sulphate, and no free oxygen remains for oxidising the rope, therefore the action of the mine water on the rope is relatively slow, as it is attacked only by the free sulphuric acid. The part of the rope exposed to the action of the hot moist atmosphere of the shaft was rapidly oxidised.

In conclusion, we may say that for the moment the best method known for protecting wire ropes from the corrosive action of the mine water and mine atmosphere is to use galvanized wires, or, better, a cover of neutral and well-distributed tar.

We see also that the ropes must be handled with care, as every break or wound of the protective zinc cover may become a centre of oxidation and rapid destruction of the wire. The galvanizing process must be done with the greatest care, so that the zinc should cover the wire without interruption.

The same idea must be considered when tarring a rope, as we have seen that to tar the surface wires and leave the internals unprotected does not help much.

Mr. WILLIAMS: I regret that during the war the contents of my laboratory in Simmonds Street were completely destroyed. I know that the analysis of that water showed about 120 grains of SO_2 per gallon. It was a well-known fact that the water of Loch Katrine is the purest in the world, and yet it is positively a fact that if you use that water on a new boiler it has a bigger corrosive effect than any other water in the world. When a boiler is started in Glasgow they use bad water at the commencement, and then use Loch Katrine water, and it does well for many years. I think the reason for that is the water being so pure it contains more oxygen than impure water, and it bears out what my friend Prof. Prister has said, that it is simply a question of oxidation.

Mr. BERRY: At the Catlin Shaft, about 3,600ft. deep, when we started to unwater the shaft after the war the water was 400ft. from the surface. The bell lines, which were galvanized, had been left hanging in the shaft since the war started. About 200ft. from the surface there was a stream of water coming in there—spring water. It never affected the boilers. In drawing this bell line to the surface the rope was very pliable, and there was no rust for about 800ft. where the rust first commenced. At 1,500ft. down the shaft there was another spring coming in, which was a different kind of water. As we approached this depth on bending the rope all the wires broke short. Yet all the wire was there and the galvanizing was there. We kept on winding up the wire, and when we got within 300 or 400ft. from the bottom the wire was good again, and very pliable, but covered with slime or mud, which had no doubt prevented the acids attacking it. From this I think it is clear that galvanized wire will stand in good water. With regard to the sample which Mr. Thomas produced, where he says the internal corrosion was somewhat surprising, I would like to ask him under what condition the rope was working? Was it a vertical shaft or an incline shaft?

Mr. THOMAS: It was incline and vertical both.

Mr. BERRY: In the case of a sinking shaft, when the bucket gets free from the runners the ropes spin a lot and open themselves out, and allow the water to get in and start corrosion. Mr. Williams' suggestion about putting lime in is very interesting. I am up against a little trouble in the property where I am at present, owing to the pipes on the surface choking up with lime scale on the return water pumps. I think it is quite possible we would have the same trouble in the mine.

The PRESIDENT: With reference to Mr. Berry's last remark about lime choking up pipes, I know of one mine they tried adding lime underground and they had the same trouble as Mr. Berry mentions. They had a good many thousand feet of small pipes choked up, and they came to the conclusion that it was just as bad to choke their pipes up as to have them corroded away.

Mr. MAYHEW: Might I ask Mr. Berry if he found the galvanizing on the wire intact where he found the wire brittle?

Mr. BERRY: The galvanizing was present when the rope came out. Of course, after it lay there a number of weeks that part which broke so easily rapidly corroded, but the part which was pliable is still there with all the galvanizing on it.

Mr. MAYHEW: You found the wires absolutely brittle inside, and yet the galvanizing was quite intact. There was no pitting in the galvanizing. I would have suspected that the galvanizing would have disappeared from these wires.

Mr. WILLIAMS: Was it very low carbon steel?

Mr. BERRY: I cannot give you any details as regards that. Perhaps Mr. Mayhew can give the information, as the rope at that time for bell lines was mostly Cradock's.

Mr. MAYHEW: I may tell you that all these bell wires are made of common Bessemer steel.

Mr. WILLIAMS: I really think this is a very important question, and I hope you are not going to peremptorily close the discussion.

Mr. DOCHARTY: I think it would be a great pity to close this discussion to-night. We have had some very valuable information brought before us. One point which struck me particularly was with reference to certain ropes, the wires of which showed very little appreciable wear, but under the microscope the structure of the steel, as exhibited by the broken ends, was proved to vary considerably at different points in the same wire. I have had an unique experience in this direction during the last few years. This is with regard to the rope used on a Whiting hoist at the mine where I now am. We have been unable up to the present time to get the rope to last longer than 4½ months. While that rope starts with a factor of safety of about 8, the remarkable feature is that for about the first four months the amount of deterioration with regard to the wear of the rope is almost infinitesimal. I may say that several ropes we had on there did not show a broken wire for the first four months. On making the usual weekly examination after the rope had been on four months, we found six broken threads, but they were all so far apart, so isolated, that we did not take any notice of it.

On the following Sunday we examined the rope again, and we found 26 broken wires. This is within one week, 20 wires broken. The rope was not used for hoisting men, and we left it on for another week. Before taking the rope off we examined it and found not less than 68 broken wires. The point that struck me about this was that there must be some action going on in that rope which the rope is able to stand up to a certain point, and when that point is reached it begins to rapidly deteriorate. This is not the only case. We have had about six ropes, some from different makers, and they have all gone in the same manner. It seems to me there may be something in the fact of the great difficulty of manufacturing steel which is absolutely homogeneous. It may be that the free graphite gets into selected places in the wire during the process of the drawing of the wire, and that in time causes the rope to break. It is peculiar that these particular ropes should all last the same length of time before showing wear, and all begin to deteriorate rapidly after that time has been passed. With reference to another matter Mr. Thomas has spoken of—the

action of acid water on cast iron—I had a remarkable experience with this, which might be held up as a warning to all of us. On examining our station pumps some time ago, I found that on the plungers of the pumps a shoulder had formed at the top and bottom of the stroke. I took the size at or near the centre of the plunger and had neck and gland bushes cast and machined up approximately to the size, with the object of having as short a shut down as possible. In putting these plungers in the lathe, instead of them cleaning up as I anticipated, they cleaned up ¼ in. smaller. The corrosion had eaten so much into the iron that we had to cut this ¼ in. off, and this notwithstanding the fact that the surface looked as if the plungers had not corroded at all. There is another matter Mr. Williams has raised, with reference to putting lime in the mine water. I was rather interested in this, as some time ago I strenuously advocated the addition of lime to mine water at the City and Suburban, but I neglected the point Mr. Berry has raised with reference to the silting up of the pipes. We get 250 lbs. static pressure on each of our pumps, and a 6 in. pipe at or near the pump barrels will silt up to 1½ in. to 2 in. bore in three or four months, and that without adding any lime to the water at all. The silt forms an artificial stone, which has to be cut out with a sledge hammer and chisel, so I think the addition of lime to the water is not feasible unless very carefully looked after.

MR. JOHNSON: Mr. Docharty has given us the case of a rope lasting only 4½ months. What is the number of bends that rope makes per trip? With reference to lime silting up the pipes, I thought that was due principally to the super-saturation of water with lime. When Mr. Williams added feeding lime into the mine water, he advocated only such a small addition as would neutralise the acid.

MR. DOCHARTY: The rope takes about ten bends altogether. I have thought myself that it is just possible that the strain of the wire is worked up from probably 100 tons breaking strain to 150 or 160 tons, and this point is reached after three or four months, and that is probably the reason the wire deteriorates so rapidly after that time.

MR. MAYHEW: At the collar of the shaft there is a very small pulley, which has a very big bend, and the friction caused by that small pulley will account for a good deal of the wear and breaking up of the wires on that rope.

MR. DOCHARTY: The particular rope I am referring to ran on the larger pulleys, and the most remarkable feature is that the broken wires on this rope are practically the same size as they were originally. They are scarcely worn at all, and yet they are broken clean through, showing, I think, that the strain on the wire is rapidly worked up.

MR. MAYHEW: I quite agree with Mr. Docharty that the wire is worked up to a very high breaking strain, till it comes to a particular point.

The PRESIDENT: With reference to keeping this paper open for discussion, I think you will agree with me that we cannot do so. We asked Mr. Thomas to give us these special tests because they were of such great interest and importance to every member of the engineering profession. In about two months' time Mr. Vaughan is reading a paper on the deterioration of wire ropes, which will open up the question in all directions and afford ample scope for discussion. In the meantime, the subject having been started, our members will no doubt collect a large amount of data before the paper is read and give us the benefit of their experience.

REPLY (COMMUNICATED).

MR. W. S. THOMAS: I feel sure that you will all join with me in thanking our visitors and members who have given us such an interesting discussion on the subject of "Corrosion in Wire Ropes."

The question of corrosion in pipes has been mentioned as a kindred subject, and possibly this is one which gives the engineer more trouble than the corrosion in ropes, though in most cases it is less serious, except where there is a chance of a jet of acid water being projected on the rope, as mentioned by Mr. Whittome. It might be of interest to know that in the Copper Mines of Butte City, Mont., where they pump water containing sulphate of

copper, they had great trouble with the pipes until the simple expedient of lining the pipes with wood was suggested. This wood lining was put in the pipes the same way as the staves of a barrel with lead gaskets at the points where the pipes were joined. When the water was put in, the wood swelled and became water-tight, thus preventing the acid water from getting at the pipes. This was found to overcome the trouble almost entirely, and it might be effective in many cases here.

The peculiar action of the water on the galvanized bell-wire, noted by Mr. Berry, would seem to confirm the theory that the structure of a steel wire may be changed without any of the usual signs of corrosion being shown on the surface.

There is no doubt that the water of the Crown Reef has a greater corrosive action than would be due to the same percentage of free sulphuric acid in other water, and it perhaps has a greater action than even a 10 per cent. solution of sulphuric acid.

Some time ago the experiment was made of putting wires which were coated with a special acid-resisting coating in the Crown Reef water. The coating had been subjected to a solution of sulphuric acid without affecting it, but in this water it lasted no longer than an ordinary uncoated wire of the same size. Mr. C. J. Price, who was manager of the Crown Deep at the time, said that acid-proof coverings were never effective in this water. He had tried coated steels which had actually been brought out to the mine in a solution of sulphuric acid, and they had failed to stand in the water. It is unfortunate that the analysis of the water cannot be obtained just now, or it might be possible to form some theory as to the particular constituents which cause this rapid action. The oxidation theory as brought forward by Prof. Prister, Mr. Williams and Mr. Mayhew, may be the cause of the greater action noted. The searing action of the water due to the fall, as mentioned by Mr. Vaughan, would not seem to wholly account for it, because in that case any mine water falling on a plate would cause the plate to rapidly deteriorate. There are many instances of mine water falling against plates or pieces of steel in other mines, and these often remain unchanged for years. The oxidation theory could be made to account for the greater reduction in diameter of the wires just at the point where the string was tied. The string was fairly thick and the water falling over the knots would cause an air-space to be formed just behind the knots. You will note that in most of the wires greater reduction is noticeable on one side where the string is attached than on the other. The strings were tied near to the middle of the wires in each case, but it is quite possible that the wires would hang with one end down and the other up during the whole time they were in the water. The air-space formed behind the knot and at the points where the wires were bent would allow the oxidation to take place more rapidly than at other points.

There is no doubt that if the water at Mr. Docharty's mine (the City and Suburban) were to get on the ropes it would have a very bad effect, but the effects noted by him in the Whiting Hoist ropes are probably due to other causes than corrosion. The increase in strain of the wire due to friction and shocks, as mentioned by Mr. Docharty, is more likely the cause than a corrosive action. I am very pleased to learn that Prof. Prister so strongly recommends galvanizing as a protection against corrosion. Speaking generally there is no reason why the zinc coating should be chipped or broken off in the parts of the wires not subjected to wear. In the small places where for any reason the zinc coating is chipped off, the grease will in all probability protect the wire at those points. On the outer wires the wear will soon remove all the zinc coating, but according to Prof. Prister's theory the oxidation process will not take place where the rope is subjected to friction.

It is certainly worth while to try some galvanized ropes here in places where the water is bad. I am inclined to think that they will prove successful.

Let us hope that the subject of corrosion will not be allowed to rest here, but that every possible precaution will be taken to prevent corrosion of ropes taking place on these fields.

Extract from "Glückauf," Aug. 1st, 1903.

ROPE STATISTICS.

Ever since 1872 a careful record of ropes used for hoisting purposes in mines has

been kept in the Dortmund District, Germany, and Table No. 1 gives the number of ropes under record.

TABLE No. 1.

Year.	No. of Mines.	Flat Ropes.				Cylindrical Ropes.		Total No. of Ropes.
		Steel.	Wrought Iron.	Manilla.	Hemp.	Steel.	Wrought Iron.	
1872	59	1	28	9	1	6	69	114
1873	76	1	26	9	—	23	97	156
1874	92	4	30	14	2	42	106	198
1875	97	8	23	5	4	74	112	226
1876	91	11	11	6	1	85	103	217
1877	85	17	10	3	—	81	67	178
1878	90	28	2	5	—	102	64	202
1879	78	23	3	3	—	99	44	172
1880	79	19	2	8	—	106	35	170
1881	76	20	6	1	—	97	41	165
1882	89	25	4	4	—	126	35	194
1883	85	20	1	4	—	138	24	187
1884	85	30	—	3	—	139	18	190
1885	86	37	—	5	—	163	26	231
1886	95	33	—	3	—	161	7	204
1887	91	32	—	4	—	156	9	201
1888	101	45	—	1	—	201	2	249
1889	99	48	—	3	—	181	7	239
1890	96	45	—	2	—	196	3	246
1891	111	46	—	2	—	229	7	284
1892	96	52	—	1	—	210	1	264
1893	106	47	—	2	—	233	1	283
1894	101	54	—	—	—	231	1	286
1895	110	51	—	—	—	226	2	279
1896	105	39	—	—	—	231	—	270
1897	107	37	—	—	—	262	—	299
1898	116	53	—	—	—	316	—	369
1899	114	35	—	—	—	353	—	388
1900	121	54	—	—	—	360	—	414
1901	130	41	—	—	—	421	—	462
1902	126	40	—		—	408	—	448
Total	—	996	147	97	8	5656	881	785

Of these 7,785 ropes 281 broke suddenly during work, as shown on Table No. 2.

TABLE No. 2.

Material.		Total No. of Ropes.	No. of Ropes that broke suddenly.	Percentage of the Total.
Flat ...	Steel ...	996	51	5·12
	Wrought iron ...	147	19	12·93
	Manilla ...	97	7	7·22
	Hemp ...	8	—	—
Cylindrical	Steel ...	5656	99	1·75
	Wrought iron ...	881	105	11·95

The total breakages are distributed over the 31 years under review, as may be seen from Table No. 3.

TABLE No. 3.

Year.	Total Ropes taken off.	Number of Ropes that suddenly broke.	Percentage Total.
1872	114	22	19·30
1873	156	22	14·10
1874	198	19	9·60
1875	226	19	8·41
1876	217	15	6·91
1877	178	16	8·99
1878	202	19	9·41
1879	172	9	5·23
1880	170	8	4·71
1881	165	8	4·85
1882	194	15	7·73
1883	187	8	4·28
1884	190	6	3·16
1885	231	7	3·03
1886	204	5	2·45
1887	201	3	1·49
1888	249	9	3·61
1889	239	6	2·51
1890	246	5	2·03
1891	284	12	4·23
1892	264	5	1·89
1893	283	3	1·06
1894	286	4	1·40
1895	279	5	1·79
1896	270	5	1·85
1897	299	4	1·34
1898	369	2	0·54
1899	388	2	0·52
1900	414	6	1·45
1901	462	5	1·08
1902	448	8	1·79

It will be seen from Table No. 3 that the percentage of breakages has been steadily decreasing from 19·3 per cent. in 1872 to 1·79 per cent. in 1902, this pointing to better materials and better design.

Tables 4 and 5 fully show the life and the work done by most of the ropes taken off the engines during the years 1901 and 1902, but it is to be regretted that the diameters of the ropes are not given, for, although the cylindrical ropes show a greater amount of work done than the flat ropes, this may be due to less work for a given section. However, there can be no doubt the cylindrical ropes beat flat ropes easily in the matter of life, Table 4 showing that not one single flat rope was used for more than 800 days, while some round ropes show a life of 1,600 days and over.

TABLE No. 4.

Life of Rope in Days.	Type of Rope.			
	Flat Ropes.		Round Ropes	
	1901.	1902.	1901.	1902.
0 to 200	20	17	52	62
200 to 400	12	16	130	128
400 to 600	7	2	85	84
600 to 800	2	5	63	68
800 fo 1,000	—	—	45	21
1,000 to 1,200	—	—	13	19
1,200 to 1,400	—	—	6	5
1,400 to 1,600	—	—	4	4
Over 1,600	—	—	3	1
Total No. of Ropes ...	41	40	401	392

TABLE No. 5.

Work done during life of Rope in Ton Miles.	Type of Rope.			
	Flat Ropes.		Round Ropes.	
	1901.	1902.	1901.	1902.
0 to 15,300 ...	17	17	111	106
15,300 to 30,600 ...	9	8	60	79
30,600 to 45,900 ...	2	4	65	48
45,900 fo 61,200 ...	5	2	46	39
61,200 to 91,800 ...	1	2	51	51
91,800 to 122,400 ...	2	1	20	33
122,400 to 183,600 ..	—	4	21	12
183,600 to 244,800 ...	—	—	5	7
244,800 to 306,000 ...	—	—	5	4
Over 306,000	—	—	4	5
Total No. of Ropes ...	36	38	388	384

Table No. 6 gives the life and the work done by Koepe hoist ropes.

TABLE No. 6.

No.	Name of Mine.	Life in Days.	Work done in Ton Miles.	Reason for Removal.
1	Germania I.	425	31,400	Not safe for man hoisting
2	Germania II.	95	9,700	Wear of Wires
3	Germania II.	138	13,100	Wear of wires
4	Germania II.	155	15,700	Wear of wires
5	Zollern I.	735	133,500	Certificate expired
6	Hanover II.	399	102,000	Several wires broke
7	Shamrock	284	158,000	Several wires broke
8	Recklinghausen	315	61,200	Several wires broke
9	Centrum I.	185	24,800	Excessive wear
10	Ernestine II.	252	33,500	Not safe
11	Graf Schwerin	364	144,500	Several wires broke
12	Hansa II.	280	23,000	Not safe
13	Ver. President	388	78,500	Certificate expired
14	Centrum I.	165	19,800	Excessive wear
15	Elbe III.	472	235,000	Excessive wear
16	Ewald II.	723	243,500	Excessive wear
17	Ewald II.	319	91,500	Tackle broke
18	Ewald IV.	917	51,500	Rope too short for deeper level
19	Lothringen I.	370	114,000	Several wires broke
20	Consolidation IV.	749	169,000	Certificates expired
21	Hercules	512	57,200	Several wires broke
22	Concorda II.	354	77,000	Rope too light for increased load
23	Konigsborn III.	364	40,300	Rope deformed
24	Konigsborn III.	364	60,400	Rope badly deformed
25	Schlagel and Eisen I.	476	100,000	Several wires broke
26	Schlagel and Eisen V.	219	20,200	Rope deformed and wires broken
27	Schlagel and Eisen V.	153	12,100	Several wires broke
28	Schlagel and Eisen	46	4,050	Work stopped
29	Consolidation IV.	725	199,000	Certificate expired
30	Hibernia II.	464	71,200	Wear due to swaying in shaft
31	Wilhelmine	721	152,500	Certificate expired
32	Centrum IV.	567	7,100	Two wires broke
33	Neu Essen	430	67,000	Several wires broke
34	Konig Wilhelm	336	118,500	Several wires broke
35	Mont Cenis I.	240	57,500	Several wires broke
36	Mont Cenis II.	719	62,500	Certificate expired
37	Mont Cenis	589	120,000	Rope to be used as tail rope
38	Ewald III.	359	120,000	Rope damaged
39	Fredrich de Grosse	262	21,500	Rope broke
40	J. Deimelsberg	295	40,000	Rope too short for deeper level
41	Prosper II.	287	118,500	Several wires broke

Table No. 1 is very instructive, inasmuch as it shows plainly how the number of steel ropes increased year by year up to 1895, when all other materials finally disappeared, wrought iron being the last to go. So far crucible steel has maintained its position, but it is fair to assume that it will also be displaced by materials of greater tensile strength, as we already feel the ill effect of the deadweight due to the ropes for the deeper shafts now in course of construction or already proposed for the deep level mines on the Rand.

With regard to Table No. 5, we must explain that the odd amounts of ton miles are due to the conversion from metrical measure, the units used in the original mkg. (metre kilogrammes).

In Table 6 will be found several ropes which were removed owing to the certificate expiring, and with reference to these time-expired ropes we must state that the usual practice in the district under review is to licence ropes for two years, or, say, 700 working days only, and if the rope is to be used after that time it has to be inspected, and if found in good condition an extension may be granted.

During 1902 eight ropes broke suddenly, of which only one was a flat rope, and, as regards the causes of breaking, in two cases no explanation can be found, whilst in one case the rope broke owing to the shaft falling in. In another instance the rope broke after the loaded cage had been lifted for about 10 ft., breaking, evidently, through too heavy strains during the acceleration period, and another rope broke owing to a coil slipping on the drum. A further breakage is accounted for by over-winding—*Glückauf*, August 1st, 1903.

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"AN INVESTIGATION REGARDING THE EFFECT OF KINETIC SHOCKS ON WINDING ROPES IN VERTICAL SHAFTS."

By Mr. J. A. Vaughan (Member).

Mr. David Gilmour, President, in the chair.

In the regulations governing mining operations in many countries there occur stipulations with regard to the strength of the wire ropes used for the raising and lowering of persons. The stipulation in several cases takes the form of a compulsory minimum relation between the breaking load of the rope and the maximum load that it is required to support.

In estimating the maximum load that a winding rope has to carry in the case of a vertical shaft it has been the custom to add together the weight of skip or cage with fittings, the weight of maximum load of persons, and the weight of rope of length equal to the distance from the headgear sheave to the bottom of the shaft. The sum of these weights, considering statical conditions only, is the maximum load that the rope has to support, and in the working life of the rope its condition must not be allowed to deteriorate to such an extent as to lower its breaking load to less than a certain multiple of this amount. This multiple commonly known as the "factor of safety," of the rope, is fixed at 6, or some higher figure.

If W_s denote the weight of cage, fittings and load (in tons of 2,000 lb.)

L_m denote the maximum depth of winding (in feet)

a denote the sectional area of wires in the rope (in sq. in.), the maximum statical load on the rope will be:—

$$W_m = W_s + \frac{L_m a}{500} \text{ tons}$$

(this accounts for weight of rope by allowing 4lbs. per ft. per sq. in. wire section.)

At a "factor of safety" of 6, the reserve of strength in the rope will be

$$5W_m = \left(5W_s + \frac{L_m a}{100}\right) \text{tons.}$$

It is noticeable that this reserve of strength increases with L_m, and, in the case of a deep-level mine, the initial reserve of strength to secure this minimum reserve during the life of the rope becomes so large that single stage winding with parallel ropes borders on the impracticable. This reserve of strength also increases with a, so that for a larger rope a greater reserve is required.

The ultimate strength of the steel used in the construction of a rope for a definite load will vary inversely as *a* so that a rope of higher grade of steel is permitted to carry a lower reserve of strength.

Imagining for a moment that similar loads of (say) 7 tons each—consisting of 6,000 lbs. conveyance and 8,000 lbs. contents—have to be hoisted from two shafts, one of 1,500 and the other of 6,000 ft. in depth. Supposing the ropes used in these two instances are of the same grade of steel (120 tons per sq. in.), then, if they are compared at a time when they each have the minimum factor of safety of 6, it will be found that the reserve of strength in the case of the 6,000 ft. rope is approximately 2½ times that existing in the 1,500 ft. rope. The latter, when the full skip is at the bottom of the shaft, will be loaded to within (say) 45 tons of its breaking load, while the former retains a margin of about 110 tons. The comparison is shown in Diagram A, as is also the variation of the factor of safety during the two hoists.

To consider this subject at better advantage it is necessary to discover what various contingencies this minimum reserve of strength is designed to meet; and, to clear the way, it may be observed that it is evidently *not* prescribed as a provision against the deterioration due to work and working conditions, for winding ropes are expected always to possess while in use this minimum reserve. It certainly ensures provision for unknown faults in the material or construction of the rope, but for this purpose, with rope of good manufacture and tested by sample, a standing minimum reserve of strength for all depths would suffice, especially so if periodical tests were conducted of the whole rope to (say) double the statical loading, and the elongation carefully noted.

A very necessary provison is also evidently intended to cover the extra stress due to the rope being bent over sheaves and drums. This being so, and this subject having recently been so thoroughly and ably treated by Professor J. Hrabak in his treatise "Die Drahtseile," we are now in a better position than before to assign a proper reserve of strength for each particular case, taking account of the ratio D/d (where D is the diameter of the sheave or drum and d the diameter of largest wire in rope), of the construction of the rope, and of the class of material used in its construction, rather than of such matters as the depth of the shaft, the total sectional area of wire in the rope, or the load to be hoisted.

It appears most probable, however that the major portion of the reserve of strength provided in a winding rope is for the purpose of meeting the kinetic shocks that are always incidental to winding operations.

Manufacturers of wire rope, while doubtless fully appreciating the effect of the stresses due to kinetic shocks, do not in their published information take the public very deeply into their confidences. They are dealing with intending users, and have, of course, to allow for working wear in the initial factors of safety that they suggest. The general values advised in their Catalogues are:—10 for quick winding in vertical, or steeply inclined, shafts; 8 for quick winding on moderate gradients; and 6 for crane work. Reading between the lines, it is apparent that large allowances are here intended to meet kinetic shocks, for the values of the relative working wear would be more likely to be in the inverse order of the above quoted numbers. It is quite possible, however, that the lowest value (of 6) proposed for crane work is partly in consideration of the less serious results of any failure of the rope in this class of work.

Kinetic shocks occur, or are likely to occur, to the most dangerous degree in cases where rapid winding is performed in vertical shafts, and the writer proposes now to investigate their effects with the view of endeavouring to decide how far these effects can be provided against by means of a reserve of strength in the rope.

In winding operations—which include hoisting and lowering, accelerations and retardations—kinetic shocks are occasioned by alterations in the relative velocity of the rope and load. In hoisting and lowering at uniform speed this relative velocity is zero, but, with the appliances used for winding, no starting or stopping, and no acceleration or retardation, can be effected in practice without an alteration of the relative velocity. As will be shown later, an elastic prismatic body, such as a wire rope may be supposed to be, if subjected, when at rest or when in uniform motion, to suddenly applied forces in the direction of its axis, transmits the stresses due to these forces by waves, which so far as they travel affect by retardation or acceleration the several

atoms of the body. Thus, during the action of a kinetic shock, all the atoms of the rope are not moving with the same velocity—in fact, each may have a velocity differing from that of any other atom. So that, in speaking of the relative velocity of the rope and its load, the velocity of *any* cross section of the rope should be understood to be included in this relation, and, consequently, a state of rest or absolutely uniform motion cannot be said to be established until *every* cross section of the rope has zero relative velocity with respect to the load.

Premising that the various causes of kinetic shocks and a comparison of their effects can be better considered after an investigation into the results attendant on some one particular case, enquiry will now be directed to the case of a rope which is suddenly stopped while lowering a load with uniform velocity. The enquiry will be divided into three stages so as to render evident the comparative significance of weight distributed in the rope or concentrated in the load.

STAGE 1.—A load W_s is being lowered with uniform velocity v in a vertical direction by a rope, assumed to be weightless, of sectional area k. Modulus of elasticity E.

When length L of rope (measured when unstretched) is paid out the motion is suddenly stopped.

What is the maximum stress at the upper cross section of the rope?

Let x_0 = the statical elongation in L due to W_s, so that $W_s = x_0 \frac{E\,k}{L}$

Let x_1 = the further elongation at time "t" after stoppage.

Let $x = x_0 + x_1$ = the total elongation at time "t" after stoppage.

The force retarding the downward motion of W_s at any time after the stoppage is dependent on the difference at that moment existing between the tension in the rope and the weight of W_s, so that an equation of motion may be stated thus:—

$$\frac{W_s}{g}\frac{d^2 x_1}{dt^2} = W_s - \frac{E\,x\,k}{L}$$

$$= W_s - \frac{E\,x_0\,k}{L} - \frac{E\,x_1\,k}{L}$$

$$\therefore \frac{d^2 x_1}{dt_2} + g\frac{E\,k}{W_s\,L}x_1 = 0 \quad \text{———} \quad (1)$$

Let solution be $x_1 = a \operatorname{Sin} qt$, so that $\frac{dx}{dt}$ $= a\,q \operatorname{Cos} qt$, and $\frac{d^2 x_1}{dt^2} = -x\,q^2 \operatorname{Sin} qt$.

Substituting these values in equation (1) we obtain:—

$$-q^2 a + \frac{g\,E\,k}{W_s\,L}a = 0 \quad i.e., \quad q^2 = \frac{g\,E\,k}{W_s\,L}$$

$$\text{Thus } x_1 = a \operatorname{Sin}\left(\sqrt{\frac{g\,E\,K}{W_s\,L}}\,.\,t\right).$$

When $t = 0$. $\frac{dx_1}{dt} = \frac{dx_0}{dt} = v$

$$\therefore v = a\,q \operatorname{Cos} q\,t = a\sqrt{\frac{g\,E\,k}{W_s\,L}} \operatorname{Cos}\left(\sqrt{\frac{g\,E\,k}{W_s\,L}}\,.\,t\right)$$

$$= a\sqrt{\frac{g\,E\,k}{W_s\,L}}, \text{ since } t = 0$$

$$\therefore x_1 = v\sqrt{\frac{W_s\,L}{g\,E\,k}}\,.\,\sin\left(\sqrt{\frac{g\,E\,k}{W_s\,L}}\,.\,t\right)$$

This gives a maximum value of x_1 :— x_1 max. $= v\sqrt{\frac{W_s\,L}{g\,E\,k}}$

This occurs at time. $t = \sqrt{\frac{W_s L}{g E k}} \cdot \frac{\pi}{2}$

or $t = \frac{\pi}{2} \cdot \frac{x_1 \text{ max.}}{v}$

The time T of the full amplitude of motion of W_s is evidently equal to 4 t.

$$\therefore T = 4t = 2\pi \cdot \frac{x_1 \text{ max.}}{v}$$

The maximum pull P on upper cross section corresponding to extension

$x_{1 \text{ max}} = v\sqrt{\frac{W_s L}{kgE}}$ will be $P = x_{1 \text{ max}} \cdot \frac{Ek}{L}$

i.e. $P = v\sqrt{\frac{W_s L}{kgE}} \times \frac{Ek}{L} = vk\sqrt{\frac{W_s E}{kLg}}$

and corresponding stress $S = v\sqrt{\frac{W_s E}{kLg}}$

The rope being supposed to be weightless, the stress, during the motion of W_s downward, which varies from 0 to $v\sqrt{\frac{W_s E}{kLg}}$ (and evidently along a sine curve), is assumed to take no time to travel from one end of the rope to the other.

The statical stress $\left(\frac{W_s}{k}\right)$ remains to be added to S.

STAGE 2.—Suppose in this case the load to be not concentrated at the end of a weightless rope, but to be evenly distributed along its length. Take in fact the practical case of a heavy elastic rope of effective cross section k being lowered vertically at uniform velocity v when the motion of the upper end of the rope is suddenly arrested.

If we assume V feet per second to be the velocity with which the stress due to this stoppage travels along the rope, then, L feet being the length of the rope, the stress will take $\frac{L}{V}$ seconds to reach the bottom of the rope, and the free end (*i.e.*, the lower end of the rope) will have during this period travelled freely and uninterfered with by the stress due to the stoppage.

During this time the lower end of the rope will have travelled $v\frac{L}{V}$ feet, which distance will be the total extension of the rope due to the stoppage.

When a rope of length L feet elongates $v\frac{L}{V}$ the stress occasioned will be $\frac{E}{L} \times \frac{vL}{V} = E\frac{v}{V}$.

Now V, the rate of propagation of stress, is known to be equal to $\sqrt{\frac{E}{\rho}}$, where ρ is the density of the elastic material. Substituting this value for V, we get :—

$$S = E\frac{v\sqrt{\rho}}{\sqrt{E}} = v\sqrt{E\rho}.$$

Let W_r be the weight of the rope.

Now $\rho = \frac{w}{g}$, where w is the weight of unit volume.

Thus $S = v\sqrt{\frac{Ew}{g}} = v\sqrt{\frac{Ew}{g}\frac{Lk}{Lk}}$

$$= v\sqrt{\frac{E \text{ (weight of rope)}}{kLg}}$$

and $P = vk\sqrt{\frac{EW_r}{kLg}}$

The elongation $v\frac{L}{V} = v\sqrt{\frac{L \cdot L\rho gk}{Egk}}$

$$v\sqrt{\frac{W_r L}{kgE}}$$

That is to say, if E is the same in both cases, and if $W_s = W_r$, the stress and elongation occasioned are equal to the maximum stress and elongation in Stage 1; so that, as far as the maximum stress is concerned, it is immaterial whether the weight is concentrated at the bottom of a weightless rope or distributed as in Stage 2.

This same result can be obtained by many different methods. To take one other :—

Suppose the length of the rope to be split up into n (an infinite number) equal parts each of length L/n, and the weight of each W_r/n to be concentrated at the end of each part. Suppose the system is being lowered with velocity v and that the arresting of upper cross section occurs.

Let $x_1, x_2, x_3 \ldots x_{n-1}, x_n$ be the

elongations at time "t" after stoppage in lengths of the rope measured from the upper cross section to the ends of the several sections, while $x_1 o, x_2 o, \ldots . x_{(n-1)^0}, x_{n^0}$ represent the elongations in these lengths when the rope is moving with uniform velocity, and let $x_1{}^1, x_2{}^1, x_3{}^1 \ldots x_{(n-1)^1}, x_{n^1}$ denote at time "t" the elongations reckoned from the positions of the ends of the sections just before the stoppage of the upper cross section occurs. Then

$$\left.\begin{array}{l} \frac{L}{n} + x_1 \\ \frac{2L}{n} + x_2 \\ : \; : \; : \; : \\ L + x_n \end{array}\right\} = \begin{array}{l}\text{Distances of the lower ends of the sections} \\ \text{below the fixed point at time "t" after stop-} \\ \text{page.}\end{array} \quad \& \quad \left.\begin{array}{l} \frac{L}{n} + x_{10} \\ \frac{2L}{n} + x_{20} \\ : \; : \; : \; : \\ L + x_{n^0} \end{array}\right\} = \begin{array}{l}\text{Distances of the lower ends of the sections below} \\ \text{the fixed point when moving uniformly} \\ \text{with velocity } v.\end{array}$$

The equations of motion of each of the sections are :—

Weight down. Pull up. Pull down.

$$\frac{W_r}{ng} \cdot \frac{d^2 x_1{}^1}{dt^2} = \frac{W}{n} - \frac{E x_1 k}{\frac{L}{n}} + \frac{E (x_2 - x_1) k}{\frac{L}{n}}$$

$$\frac{W_r}{ng} \frac{d^2 x_2{}^1}{dt^2} = \frac{W}{n} - \frac{E (x_2 - x_1) k}{L/n} + \frac{E (x_3 - x_2) k}{L/n}$$

$$: \; :$$

$$\frac{Wr}{ng} \frac{d^2 x_{(n-1)^1}}{dt^2} = \frac{W}{n} - \frac{E (x_{n-1} - x_{n-2}) k}{L/n} + \frac{E(x_n - x_{n-1})k}{L/n}$$

$$\frac{W_r}{ng} \cdot \frac{d^2 x_{n^1}}{dt^2} = \frac{W}{n} - \frac{E (x_n - x_{n-1}) k}{L/n}$$

but $x_1 = x_1 0 + x_1 1$, $x_2 = x_2 0 + x_2 1$, . . . and $x_n = x_{n^0} + x_{n^1}$ and, substituting these values, we obtain :—

$$\frac{W_r}{ng} \cdot \frac{d^2 x_1{}^1}{dt^2}, = - \frac{E x_1{}^1 k}{L/n} + \frac{E(x_2{}^1 - x_1{}^1)k}{L/n}$$

$$\frac{W_r}{ng} \cdot \frac{d^2 x_2{}^1}{dt^2} = - \frac{E(x_2{}^1 - x_1{}^1)k}{L/n} + \frac{E(x_3{}^1 - x_2{}^1)k}{L/n}.$$

$$: \; :$$

$$\frac{W_r}{ng} \cdot \frac{d^2 x_{(n-1)^1}}{dt^2} = - \frac{E\left(x_{(n-2)^1} - x_{(n-1)^1}\right)k}{L/n} + \frac{E\left(x_{n^1} - x_{(n-1)^1}\right)k}{L/n}$$

$$\frac{W_r}{ng} \cdot \frac{d^2 x_{n^1}}{dt^2} = - \frac{E\left(x_{n^1} - x_{(n-1)^1}\right)k}{L/n}.$$

Let $\frac{gEk}{\frac{W.L}{n\;n}} = q^2$, and substituting we obtain :—

$$\frac{d^2x_1^1}{dt^2} + 2q^2x_1^1 - q^2x_2^1 = 0 \quad \ldots\ldots\ldots (2)_1$$

$$\frac{d^2x_2^1}{dt^2} + 2q^2x_2^1 - q^2x_1^1 - q^2x_3^1 = 0 \quad \ldots\ldots (2)_2$$

$$\vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots$$

$$\frac{d^2x_{(n-1)^1}}{dt^2} + 2q^2x_{(n-1)^1} - q^2x_{(n-2)^1} - q^2x_{n^1} = 0 \quad (2)_{n-1}$$

$$\frac{d^2x_{n^1}}{dt^2} + 2q^2x_{n^1} - q^2x_{(n-1)^1} = 0 \quad \ldots\ldots (2)_n$$

Adding all these equations we obtain :—

$$\frac{d^2x_1^1}{dt^2} + \frac{d^2x_2^1}{dt^2} + \cdots + \frac{d^2x_{(n-1^1)}}{dt^2} + \frac{d^2x_{n^1}}{dt^2} = -q^2x_1^1$$

i.e. the sum of the accelerations of each section at any particular time t after the stoppage $= -q^2x_1^1$.

If now we take t so small that the stress due to stoppage has travelled no farther than the first section, it is evident that the other sections are still proceeding with velocity v and that they have no retardation. In this case

$$\frac{d^2x_1^1}{dt^2} + o + o + \cdots + o + o + o = -q^2x_1^1$$

If now we assume the first section to be brought to rest from velocity v by a retardation increasing uniformly with distance, this retardation may be equated to $\frac{v^2}{x_1^1}$.

Thus we have $\frac{v^2}{x_1^1} = -q^2x_1^1$

or $v^2 = q^2x_1^2$ in magnitude

leading to $x_1^1 = \frac{v}{q}$.

But $q = \sqrt{\frac{gEkn^2}{W_rL}}$

$$\therefore x_1^1 = \frac{v}{n}\sqrt{\frac{W_rL}{gEk}}.$$

If P_1 is the pull occasioned by this extension in length $\frac{L}{n}$

then $P_1 = x_1^1 \frac{E\,k\,n}{L}$

i. e., $P_1 = \frac{E\,k}{L} v \sqrt{\frac{W_r\,L}{g\,E\,k}}$

$$= v\sqrt{\frac{E\,k\,W_r}{Lg}}$$

but $W_r = L\,k\,\rho\,g$, where ρ is the density of material.

$$\therefore P_1 = v\,k\sqrt{E\rho} = v\,k\,E\sqrt{\frac{\rho}{E}}$$

so that if V, the velocity of propagation of stress $= \sqrt{\frac{E}{\rho}}$ we have $P_1 = v\,k\,\frac{E}{V}$

or $S_1 = E.\frac{v}{V}$

Similarly this same value can be obtained for the stress in any section of the length, on the assumption that the sections above it are brought to rest, and the sections below it are still moving with velocity v.

This proof taken into conjunction with that previously given will demonstrate that $V = \sqrt{\frac{E}{\rho}}$, a fact that the writer had assumed in the first instance.

In his interesting paper, "On Explosions of Steam Pipes due to Water-Hammers," Mr. C. E. Stromeyer, M.I.C.E., gives a clear demonstration of the principles governing the phenomena attendant on shocks in elastic material. The writer was pleased in that paper to find data corroborative of his own calculations up to this stage in the investigation, and wishes further to thank Mr. Stromeyer for the

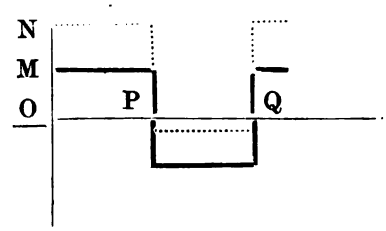
valuable advice and suggestions that correspondence with him has produced.

The equations previously stated, $(2)_1$, $(2)_2$, ...$(2)_{n-1}$, $(2)_n$, can be solved when n is taken as a small number, 2, 3, 4 or 6, and particular numerical values given to other symbols. The curves drawn in Fig. 1 show the results of calculations based on the assumptions that the weight of the rope is concentrated at 1, 2, 3 and 6 points respectively. The curves are numbered 1. 1. 1., 2. 2. 2., etc., accordingly, and the starting point of each is set off a suitable distance to the right-hand side of the origin—O to allow for the time that would elapse before the stress reached the upper end of the rope. The result for the case of the infinite distribution of the weight can in this manner be seen to be identical with the Fourier expression:

$$Z = \frac{4v_o}{\pi} \text{ Sin qt} + \tfrac{1}{3} \text{ Sin 3 qt} + \tfrac{1}{5} \text{ Sin 5 qt} + \&c.)$$

Fig. 2.

Referring to Fig. 2, Z is measured upwards from the line OQ. $OM = v_o$ and $q = \frac{2\pi}{T}$, T being the periodic time and represented by OQ.



The figure can be drawn to represent P_1 the kinetic load on the upper cross section, or x_1 the strain. If the former, then

$$Z = P_1 = \frac{4v_o}{\pi}(\text{Sin qt} + \tfrac{1}{3} \text{ Sin 3 qt} + \&c.) \text{ where}$$

$$v_o = vk \sqrt{\frac{Ew}{g}}, \quad q = \frac{\pi}{2L}\sqrt{\frac{Eg}{w}},$$

$$\text{and } T = OQ = 4L\sqrt{\frac{w}{Eg}} = 4\frac{L}{V}$$

The dotted line shows the whole load on the upper cross section, including the statical load (at an assumed value MN).

Fig. 2 shows that a sudden reversal of stress will occur at any point of the rope, from a positive to a negative value. This will be accompanied by a sudden change of motion, and on this point the remarks of Mr. Stromeyer in the above-mentioned paper may be quoted:—"A force imparts a given velocity to a given mass in a given time. If the force is of a fixed intensity, then the velocity will also be fixed, provided the mass and time are proportioned to each other. Now, the mass of the very extreme end of the spring can be imagined to be infinitely small, therefore, the time in which it acquires or changes its velocity under the influence of a definite pressure must also be infinitely small."

Regarding the value of V for steel rope, the writer leaves this to be determined experimentally, and has, for the sake of simplicity in these calculations, assumed it to be dependent on the modulus of elasticity and density of the rope, and not of the wire. Taking E as 20,000,000 lbs. per square inch, or 1,440,000 tons per square foot, and the mass of a cubic foot of rope (including the weight, but neglecting the volume, of the core) as equal to ·288/g tons, then

$$V = \sqrt{\frac{1{,}440{,}000 \times g}{\cdot 288}} = 12{,}650 \text{ feet}$$

per second (approximately).

In using this value to estimate the time required for the stress to travel along a certain length of rope, the length of the rope is taken as the distance travelled, and not the length of the wire spiral.

This wire spiral may be, in practice, of length only 5 per cent. greater than the rope, and E for the wire itself may be as great as 30,000,000, so that it is necessary to bear in mind that considerable error may be involved in the approximation.

Seeing that the maximum straining effect of the kinetic energy of a weight is the same whether the weight be distributed in the rope or concentrated at the lower end, it appears reasonable, at first sight, when considering in the case of a heavy loaded elastic rope subjected to kinetic shock, to imagine the weight of the rope as added to the load and as acting in conjunction with the load at the lower end of the rope.

The writer for some time regarded this as a near approach to the truth.

Competent authorities have advised such approximations as adding one-half, or one-third, of the weight of the rope to the load.

All these expedients to simplify calculations lead to theoretical results that apparently agree with those that practical experience lays down as correct. For instance, they show that kinetic shocks in a very short rope are highly dangerous, while the additional elongation possible in a long rope serves to considerably modify the stress due to such shock.

In fact, the result may be stated thus:—

"The dynamical stress in the winding rope, or that caused by the kinetic shock, becomes more dangerous, that is to say, of a so much greater effect, *the smaller the volume* of the stressed rope."

As for the correctness of this deduction, it appears only to be true within certain limits—these limitations depending on the length and size of rope and the weight of load carried.

In all these approximations the assumption is involved that the elasticity of the rope taken as a whole can be utilised immediately, or with sufficiently great rapidity, to moderate the effect of shocks. This assumption is not correct, for *time* has to be taken into consideration.

The concluding stage of this investigation will show how far this assertion is justifiable.

STAGE 3.—Let W_r be the weight of a heavy elastic rope lowering a load of W_s at the rate of v feet per second. When L feet of rope are out, the upper end of rope is stopped. To find the maximum stress occasioned in upper cross section.

One way to treat this problem would be to introduce the load W_s whole or in sections into the various equations $(2)_1$, $(2)_2$, up to $(2)_n$ of stage 2; this is to say, to add it on to the weight of the sections of the rope.

The equations may then be solved by inserting suitable numerical values for the symbols.

The results of this method are shown in curves 1. 1. 1., 2. 2. 2., 3. 3. 3. and 6. 6. 6. in Fig 3, and it can be seen how the stress rises as the distribution of the weight of the rope is made more complete.

However, this method, while interesting, is tedious and, at best, only approximate; the following solution is put forward as being more satisfactory.

Neglecting for the time the statical stress which will be added later, it is known from Stage 2 that the lower end of the rope, and with it W_s, will continue motion downwards at velocity v for an interval of $\frac{L}{V}$ seconds, while a pressure wave of extension $= P = vk\sqrt{\frac{EW_r}{kLg}}$ travels down the rope.

On reaching the lower end of the rope this pressure wave, in instantaneously reversing, causes an upward pull $= P = vk\sqrt{\frac{EWr}{kLg}}$ which is opposed by the kinetic energy of W_s, *i.e.* $\frac{W_s v^2}{2g}$

The lower end of the rope is thus constrained to move downwards still further with the weight W_s while the velocity of the fall gradually diminishes as the kinetic energy of the weight is expended.

In explanation of this let us consider the action between the lowest section of the rope and W_s. Imagining the rope to be composed of n (an infinite number) infinitely thin horizontal slices joined to each other with the elasticity pertaining to the rope, it is clear that the nth slice, which at the end of $\frac{L}{V}$ seconds would immediately acquire a velocity of v upwards, is forced downwards with velocity v by the action of W_s. The stress produced in this slice of the rope is evidently $2v\sqrt{\frac{Ew}{g}}$, *i.e.* 2S, of which one S keeps it from upward movement, that is to say stops it and keeps the tension P in the whole rope, while the other S gives it velocity v downwards and thus leads to a further straining of the rope. The nth slice communicates this latter stress of S to

the (n – 1)th slice and also the velocity v. In doing so the nth slice would come to rest were it not that W_s with reduced velocity v_1 moves it on. This second jerk causes stress $v_1\sqrt{\frac{Ew}{g}}$, which in turn is communicated to the (n – 1)th slice, and so on, jerk after jerk, until the whole kinetic energy of the weight is expended. The (n – 1)th slice communicates these stresses and velocities in succession to the (n – 2)th slice, and thus a pressure wave of extension, with head equal to P=kS and tail diminishing to zero, travels up the rope, which, it must be noted, is still at S stress (unrelieved) above statical loading.

The length of this wave has an important bearing on the maximum amount of stress occasioned at the upper cross section of the rope, in that, should the rope be so short that the wave after reflection from the upper cross section can return to the lower end before the motion of W_s downward is completed, it is evident that there will be a further sudden check to this motion, this involving an additional stress in the rope.

(The term " wave " is used in the sense of a semi-undulation.)

Diagram B has been drawn to illustrate the progress of the wave.

The resulting stresses, greatly exaggerated, are indicated by the reduction in the width of the rope.

Regarding this wave length it will be seen that, if the rope is sufficiently long to accommodate the wave, the arresting of the downward motion of W_s is accomplished by a force starting at value 2P and decreasing gradually to value P, for the whole rope is stressed to the amount P/k, and therefore a recoil will start when this value is reached. Thus the work done by this variable force may be equated to the kinetic energy $W_s v_2/2g$.

It is necessary now to note at what rate this force varies.

If the value of E remains constant for the stresses under consideration, and on a similar assumption with regard to ρ, then the rate of propagation of stress $V (=\sqrt{E/\rho})$ will also remain constant. This being so, the rate at which the successive atoms of the rope are jerked into motion is constant, and the retarding force at any particular time, due to imparting velocity to the lowest atom at this instant, will depend solely on the velocity of W_s at this instant.

The whole force retarding motion of W_s consists of two parts, one of which is constant, viz., P, and the other part, viz., $v_r k\sqrt{\frac{Ew}{g}}$, varies with the velocity, – which at the rth instant may be denoted by v_r.

The equation of motion may then be stated:—

$$\frac{W_s}{g}\cdot\frac{dv_r}{dt} = Cv_r + P, \qquad \text{where } C=k\sqrt{\frac{Ew}{g}}$$

$$i.e.\quad dt = \frac{W_s}{g}\frac{dv_r}{Cv_r+P}$$

Integrating between limits v_r=o and v.

$$t = \frac{W_s}{g}\int_0^v \frac{dv_r}{Cv_r+P}$$

$$= \frac{W_s}{g}\cdot\frac{1}{C}\left\{\log_e(Cv+P) - \log_e P\right\}$$

$$= \frac{W_s}{gC}\cdot\log_e\frac{Cv+P}{P}$$

$$= \frac{W_s}{gC}\log_e\frac{2P}{P}, \text{ since } Cv=P$$

$$= \frac{W_s v}{gP}\log_e 2$$

$$= \cdot 693\,\frac{W_s}{g}\,\frac{V}{kE} = \text{say, } 7\,\frac{W_s}{g}\,\frac{V}{kE}$$

During this time the pressure wave (P to O) has been travelling up the rope at the rate of V feet per second.

Thus, if W be the length of the wave, t=W/V.

and, therefore, $W\cdot 7 = \frac{W_s}{g}\cdot\frac{V^2}{kE}$

substituting the value $V^2=\frac{E}{\rho}=\frac{Eg}{w}$

*we obtain $W = \cdot 7 \frac{W_s}{wk}$, or $W.w.k = \cdot 7 W_s$

so that the weight of rope included in the wave (of length W) is equal to ·7 of the weight of the load carried.

Now w is the weight of a cubic unit of the material of which the rope is constructed. In it is included the weight of non-effective material such as soft cores, etc., on the assumption that these occupy no space. For a definite class of ropes, of similar construction, w may be regarded as a constant.

This being so, it appears that the length of the wave is dependent solely on the ratio W_s/k, which is the statical loading of the rope so far as affected by the load W_s. – k being the cross sectional area of the effective material composing the rope.

Now S has been shown to be equal to $v\sqrt{\frac{E\,w}{g}}$, with v as the only variable if E and w are constant.

Thus, for a given rope, with W_s and v fixed, the only possible factor affecting the stress due to a kinetic shock, such as is under consideration, is the length L of the rope.

The value of S itself has been shown to be independent of L, and it now remains for consideration whether $S_{max.}$ in the upper cross section is influenced by this length.

What occurs when the pressure wave reaches the upper cross section of the rope?

This point being temporarily, by supposition, a fixed point, the head of the wave on reaching it suffers reflection downwards, with the result that, as the several elements of the wave arrive and depart, they produce a double pull on, and a double stress in, the upper cross section.

When, therefore, the head of the wave is being reflected a stress of 2S is occasioned. To this must be added the stress of S already in the rope due to its own stoppage, making in all 3S above the statical loading.

As far as the pressure wave doubles on itself the stress in excess of S at any particular point in the rope is the sum of the advancing and receding values, for it has to be remembered that both produce extension, being of the same sign, and there is no nodal effect.

If the length of the rope is equal to half the wave length, i.e., if W = 2L, it is evident that, neglecting the lengthening effect of the stress on the rope, the head of the wave will return to the lower end of the rope at the moment that the tail is leaving it.

The kinetic stress in the lower cross section of the rope will be S + S + O = 2S. The load W_s will have been just brought to rest, its kinetic energy being all expended in the production of the wave. The rope will now, atom by atom, be relieved of all stress above its statical loading, the work performed consisting of the upward acceleration of W_s and of the rope itself.

Without continuing in the present paper the very important investigation of the resulting motion, it may be inferred that the velocity acquired by the mass of W_s and of the rope will allow of motion above the position of static equilibrium, this leading to a fall of these masses, the acquisition of velocity downward, and a repetition of the cycle, modified of course in a practical case, by the effects of internal and external resistances.

Considering now the effect in a rope longer than $L = \frac{W}{2}$, it is apparent that, if the length of rope exceeds half the wave length, the head of the wave will not return to the lower end of the rope until W_s has acquired some upward velocity.

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* NOTE.—This result does not differ very materially from those obtainable from the assumptions that the retarding force varies according to the time, or to the distance travelled by W_s.

In this latter case—	In the former case—
$t = \frac{W_s}{g}\cdot\frac{v}{\sqrt{3}.P.}\log_e(2+\sqrt{3})$	$v = \frac{2P+P}{2}\cdot\frac{g}{W_s}\cdot t$
$= \cdot 76\frac{W_s V}{gk\sqrt{E}}$	$t = \frac{2}{3}\cdot\frac{W_s V}{gk\sqrt{E}}$
and $W = \cdot 76\frac{W_s}{wk}$	and $W = \frac{2}{3}\cdot\frac{W_s}{wk}$.

However, be the rope ever so long, it is obvious that, as the upward motion of W_s is dependent on a wave of release that follows in the wake of the wave of stress, the whole of this latter must have returned to the bottom of the rope before the completion of the upward travel of the weight, so that at some time or another in this semi-vibration the kinetic energy which the weight W_s previously gave up to the rope will be restored to its source.

On the other hand, no additional energy can be created in the system by the wave motions, and, in the vibrations subsequent to the first collision between the rope and the weight, there will never be between the weight and any section of the rope a greater relative velocity than $2v$, which relative velocity existed between the weight and the lower section of the rope at the instant of the first collision. Consequently the effect of this first collision can never be exceeded in magnitude by that of subsequent ones. Therefore, for a length of rope equal to or greater than W/2, the maximum stress occasioned in the upper cross section will be, from theoretical consideration, equal to 3S.

To what extent in practice this stress may be reduced in the case of a long wire rope is a matter that may perhaps be capable of demonstration by means of actual experiment; but, from a consideration of the structure of such rope (which, of course, is not really a prismatic body, although assumed to be so in the previous calculations) and of the fact that extensions or contractions under stress involve the untwisting or further twisting of the elements, it may be expected that a wave of stress will materially reduce in energy while traversing a long length of wire rope.

Take now the case of a rope shorter than W/2.

Here it is clear from previous argument that when the head of the wave reaches the lower end of the rope, the motion of W_s downward will not be completed, and it will be falling with a velocity (say) v_1.

In the small section of rope at that instant receiving the velocity v_1 a stress of S exists, and a further stress of S_1, corresponding to velocity v_1, is being produced; but, owing to the return of the head of the wave the whole stress will be increased to value $2S + S + S_1 = 3S + S_1$, and the retardation of the motion of W_s will be similarly effected, that is to say the force producing retardation at that moment, viz., $P + P_1$, will be instantaneously increased to $3P + P_1$, this resulting in a more rapid checking of the velocity of W_s and a reduction of the time t to t_1.*

The wave after this second reflection, and with head now $P + P_1$, travels up the rope again. When it meets the upper cross section and suffers its third reflection the instaneous stress produced will be $2(S + S_1)$, which, added to the stress of S existing in the rope due to its own stoppage, totals $3S + 2S_1$.

If the rope is so short that the head of the wave can return a second time to the lower end of the rope before the motion of W_s downwards is completed, a further increase of stress will be produced, and it appears that the maximum stress in the upper cross section for the case of a short rope may be represented by the formula $S_{max.} = S + 2(S + S_1 + S_2 + \text{etc.})$ where S_1, S_2, S_3, etc., are each less than S, and in decreasing order of magnitude.

However, there may be a very large number of these augmentations in a very short rope. For the object the writer has in view, it is not necessary to do more here than indicate that these results lead one to expect the production of an infinitely great stress in an infinitely short rope, and that the motion of W_s will be instantaneously arrested.

In a rope shorter than W/2 the motion at W_s downwards is stopped by a series of jerks occurring each time the head of the wave returns to the lower end of the rope. Subsequently, the acceleration of W_s upwards is also accomplished in this spasmodic fashion, the number of impulses in each case depending on the shortness of the rope.

When the rope is of length equal to W/2, the wave effects due to impact synchronise with the natural vibration of W_s at the end of this length of rope.

When the rope is longer than W/2 there occurs one jerk on the upward motion of W_s.

(*) [See Diag. VI., in paper by the same author, entitled "The Factor of Safety of Wire Ropes used for Winding in Mine Shafts," page 250.—Ed.]

These effects can be plainly seen in experiments conducted with a variable weight at the lower end of an india-rubber cord or tube of length (say) over 20 feet. The upper end of the cord or tube is to be attached to the lower end of a short piece of unstretching string which has its upper end fixed to a rigid support. The whole system is lifted a certain height and then dropped,—the short length of string providing for a sudden stop of the india rubber.

The writer experimented thus, and allowed a drop of 1·55 ft. leading to a velocity of 10 ft. per second.

With these dimensions the stretching of the india-rubber cord is sufficiently great, and the vibrations and jerks sufficiently slow, to allow of personal observation and detection.

A measurement of the actual extensions for various lengths of cord and for various velocities is very interesting and instructive.

A similar combination of string, india-rubber cord, and weight can also be attached to the hook of a spring balance and arrangements made for varying the length of india-rubber, so as to test the kinetic shock for short and long lengths of cord. A small india-rubber marker, shaped thus ▽, threaded on the head of the index pointer of the balance, in its movement over the dial, thinly smeared with red-lead paint, gives a ready indication of the maximum effects.

The records need to be translated, keeping in mind the inertia of the various parts of the apparatus, and the doubling effect of sudden shock on the spring of the balance, and even then are of little value as affording a comparison with what might happen in the case of a steel wire or wire rope receiving a similar kinetic shock, for the reason that the V for india-rubber increases so enormously with the addition of stress.

Still, experiments such as those just described are of some value, and the results readily show, without any correction or adjustment, how in varying lengths of cord the kinetic stress increases rapidly from a certain minimum point (corresponding to $L = W/2$) to very large values as the cord is shortened, and how, as the cord is lengthened above the value $L = W/2$, the kinetic stress keeps fairly constant.

By dropping varying lengths of the india-rubber cord without any weight attached at lower end, it can also be plainly seen by the indications of the spring balance that the value P (which $= vk\sqrt{\frac{Ew}{g}}$) is independent of the length of cord.

Many other experiments of a more practically useful character will doubtless suggest themselves to engineers interested in the subject of the effect of kinetic shocks in winding ropes, the utility of such experiments naturally increasing according to the extent to which the actual working conditions are reproduced.

Professor J. Hrabak has experimented with many winding ropes—hanging in vertical shafts and loaded up to their usual working loads—and has thus found practical values for the modulus of elasticity of both, old and new ropes.

It would be interesting in these mining fields to carry out similar experiments, better still, to extend them so as to discover V the rate of propagation of stress in the rope, and also to see at what rate such stress diminishes while travelling. Uncertainty on these points requires to be cleared up, and this can only be effected by carefully conducted tests on long lengths of wire rope stretched to their working loads or thereabouts.

It is the writer's earnest hope that some members of this Association, possessed with the necessary resources and opportunities, will carry this matter beyond its present stage of improvised laboratory trial.

Returning now to the results obtained by theoretical investigation, and which rough practical experiments prove to be apparently true, it has been shown that for lengths of rope weighing less than ·35 of the load that they carry the maximum kinetic stress at the upper cross section, due to a sudden stop in lowering, increases rapidly as the length is shortened, while for a length equal in weight to (or greater than) ·35 of the load, the kinetic stress is constant and is equal to $3v\sqrt{\frac{Ew}{g}}$

If, now, we add to these results the statical stress, we obtain the whole stress at the upper cross section. This has been done and the values (in tons of 2,000 lbs. per sq. in.) for all lengths of rope up to 6,000 ft.

are plotted on Diagram C. The data used in the calculation are:—

$v = 10$ ft. per sec. $V = 12{,}650$ ft. per sec.
$k = 1$ sq. in. $= \frac{1}{144}$ sq. ft.
$E = 20{,}000{,}000$ lbs. per sq. in. $= 10{,}000$ tons per sq. in., or $= 1{,}440{,}000$ tons per sq. ft.

$w = \cdot 000166$ tons in the case of a cub. in., or $\cdot 288$ tons in the case of a cub. ft.

The statical factor of safety at various depths is also shown, and for this purpose the ultimate strength of the steel is taken at 120 tons, but reduced to 105 tons as allowing for "laid-up" efficiency.

The large kinetic shock that we have been considering, due to a sudden stop of the upper end of the rope while lowering a load, is only one out of many dangerous effects that are, within the bounds of possibility, attendant on winding operations.

Exception may, of course, be taken to the assumption of a *sudden stop*, but its justification lies in the fact that in practice an attempted gradual ratardation consists in the application of at least one sudden check to the speed, and a sudden loss of, say, v ft. per sec. velocity—the effect of which may be calculated from the formula previously established—the velocity being purely a multiplier of the kinetic stress, and the relation $S = 3v\sqrt{\frac{Ew}{g}}$ holding true for all velocities.

If a number of small checks of speed are applied in rapid succession to the rope, there is the possibility of their producing an effect equal to the sum of their separate effects.

For instance, if there is a sudden check of v ft. per sec. velocity, the duration of its first action on W_s will be $t = \cdot 7\frac{W_s}{k}\cdot\frac{V}{gE}$. This time solely depends on the ratio W_s/k, and if the rope is longer than W/2, and if the checks of velocity are sufficiently rapidly applied, so that the shock of the second arrives at W_s before the effect of the first is completed, the shock of the third before the completion of the effect of the second, and so on, it is clear that the cumulative result is obtained. This time, for the rope and weight data illustrated in Diagram C would be slightly under 1/5th second, so that, with a good engine driver, and all appliances in proper working condition, the possibility of the cumulative effect does not on consideration appear to be very great. However, the chattering of a brake might easily lead to the application of a series of shocks with the necessary rapidity.

If the rope is shorter than W/2 the time of descent of W_s is decreased, and there is more certainty of the effect of the first shock being completed before the second shock arrives, but as has been shown before, this time can only be shortened with an accompanying increase of stress.

These effects due to jerky retardations, and the analogous effects of intermittent accelerations, demand a closer investigation than the writer has been able to deal with, for herein lie the reasons for the excessive "pumping" action of a winding rope, which action leads not only to undue strains on the rope, but to the unexpected gripping of the safety catches, a "nang-up" of the cage, and other derangements of winding practice which may possibly involve dangerous consequences.

If it were possible to stop the motion of the rope and load by means of an absolutely gradual retardation starting at zero, then it can be readily seen that for very gradual actions the maximum stress produced in a long rope will be greater than that produced in a short rope, but the maximum stress in the former case would fall far below that calculated for the sudden stop. If, however, the time of action is shortened up, the stress in the short rope increases much more rapidly than in the case of a long rope, and a minimum value will appear at some depth dependent on the data introduced in a specific case.*

* * * * * * *

Here concludes, for the present, the writer's investigation. The following deductions appear justifiable:—

(1) Within a certain limit of depth from surface, this depth depending on the ratio W_s/k, a winding rope is stressed more severely than at greater depths by a kinetic shock of given magnitude.

(2) From this limit of depth to the surface the stress for a given kinetic shock increases as the rope is decreased.

(3) The portion of winding rope nearest to the load (*i.e.*, the portion that never

(*) [The portion of this paper which is excluded will be found as an Appendix to a subsequent paper by the same author, entitled "The Factor of Safety of Wire Ropes used for Winding in Mine Shafts," see pages 240—251.—ED.]

winds on the drum) is a part that has to bear the sudden application of *every* kinetic stress due to changes in the kinetic energy of the load, and may be expected to deteriorate more rapidly than other portions of the rope.

(4) For all depths greater than the dangerous limit the maintenance of a standing reserve of strength in the rope appears a suitable provision against the effects of kinetic shock.

In explanation of this deduction, to which the writer imagines some importance attaches, it requires to be remembered that, although the enlarging of the rope does decrease the depth of the dangerous limit, it *does not reduce the kinetic stress* for depths beyond this limit.

With a seven-ton load, if the rope were increased from 1·27 in. diameter to 1.6 in. diameter, the sectional area of the wires would probably be increased by 50 per cent., the weight per foot would also be increased by 50 per cent., while the depth of the dangerous limit would be reduced from 1,823 feet to 1,213 feet—that is, by one third.

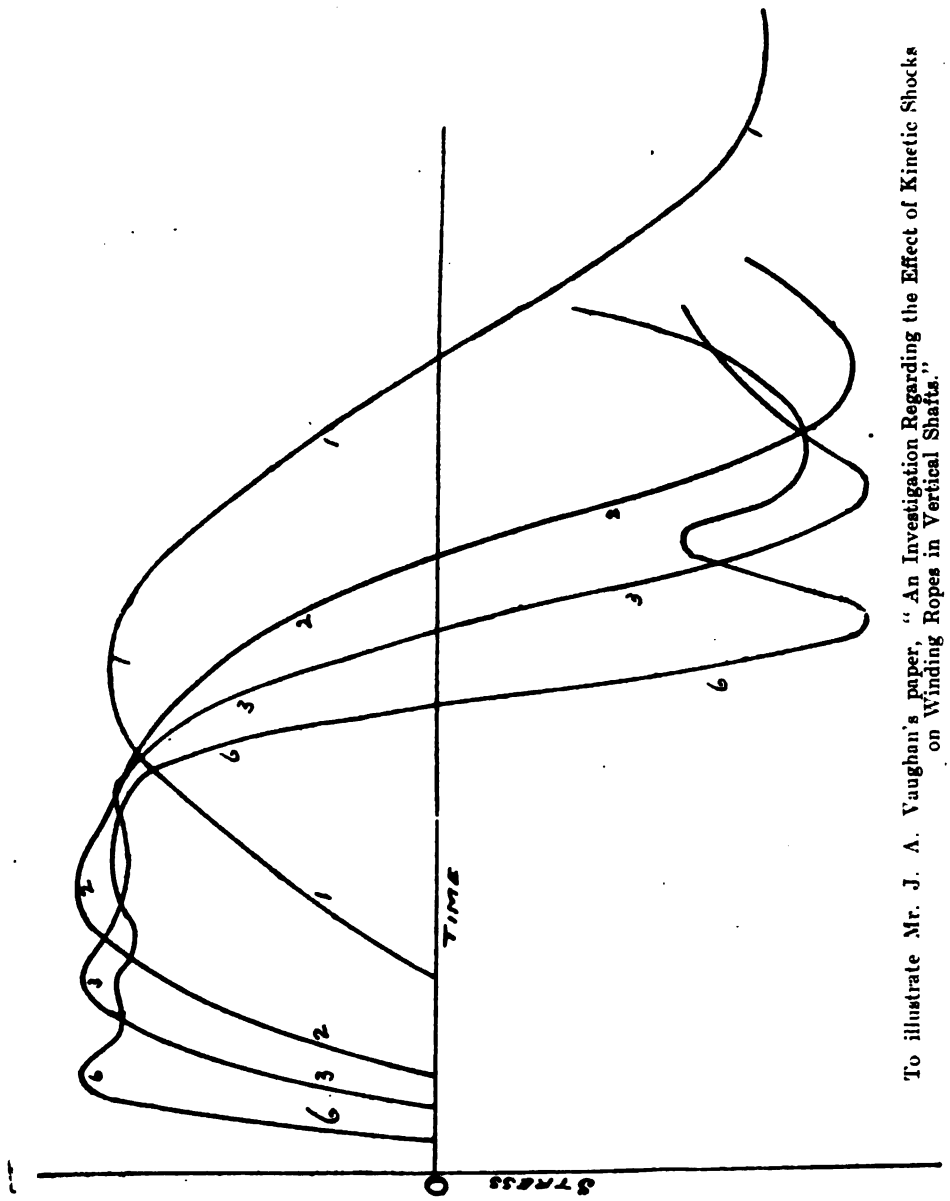
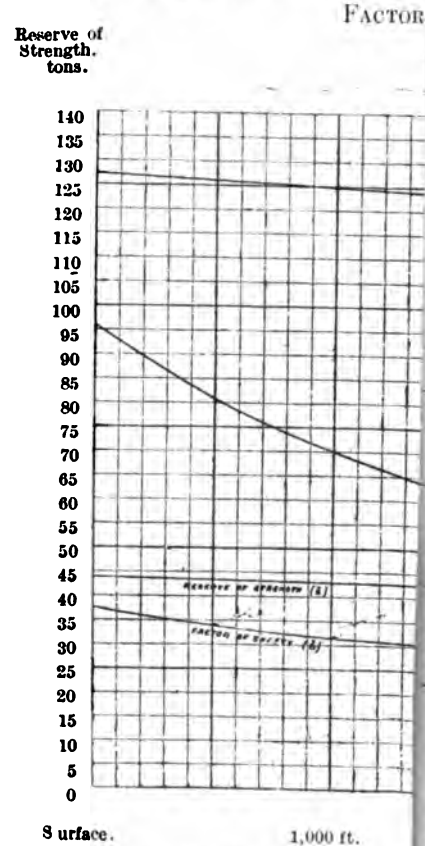


FIGURE 1.

To illustrate Mr. J. A. Vaughan's paper, "An Investigation Regarding the Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts."

DIAGRAM A.—SHOWING RESERV
FACTOR



To illustrate Mr. J. A. Vaughar

ROPE FOR 6,000 FEET.

CARRYING LOAD OF 7 TONS

Depth in Feet.	Weight.	Factor of Safety.	Reserve of Strength.
6,000	22·39 tons.	6·002	112·01 tons
5,000	19·82 ,,	6·78	114·58 ,,
4,000	17·26 ,,	7·78	117·14 ,,
3,000	14·695 ,,	9·15	119·7 ,,
2,000	12·13 ,,	11·07	122·27 ,,
1,000	9·565 ,,	14·05	124·83 ,,
Surface	7·00 ,,	19·2	127·4 ,,

Weight of Rope per foot = 5·13 lbs.
Breaking Load of Rope = 134·4 tons.

ROPE FOR 1,500 FEET.

CARRYING LOAD OF 7 TONS.

Depth in Feet.	Weight.	Factor of Safety.	Reserve of Strength.
1,500	8·455 tons	6·02	42·468 tons
1,000	7·97 ,,	6·38	42·953 ,,
500	7·485 ,,	6·8	43·438 ,,
Surface	7·00 ,,	7·274	43·923 ,,

Weight of Rope per foot = 1·941 lbs.
Breaking Load of Rope = 50·923 tons.
Ton = 2,000 lbs.

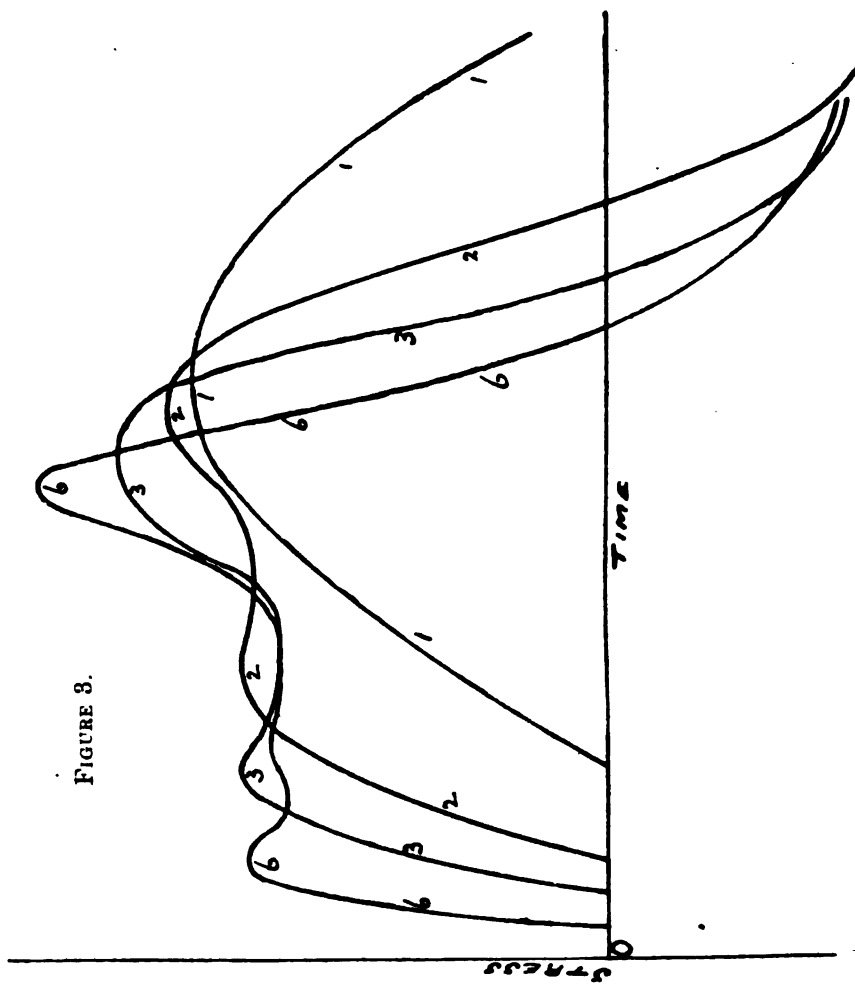
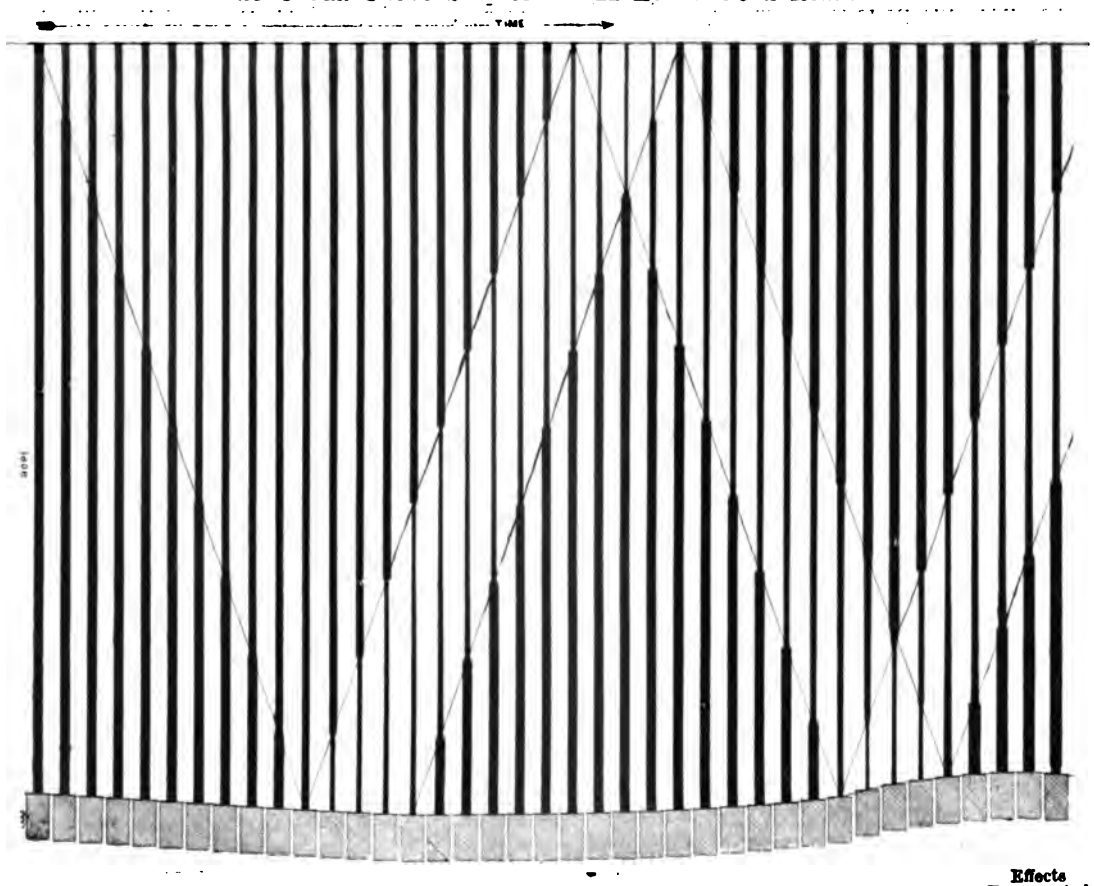


FIGURE 3.

To illustrate Mr. J. A. Vaughan's paper, "An Investigation Regarding the Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts."

F

DIAGRAM B.—SHOWING STRESS OCCASIONED IN A WINDING ROPE WHEN SUDDENLY STOPPED AT UPPER CROSS SECTION WHILE LOWERING A LOAD.

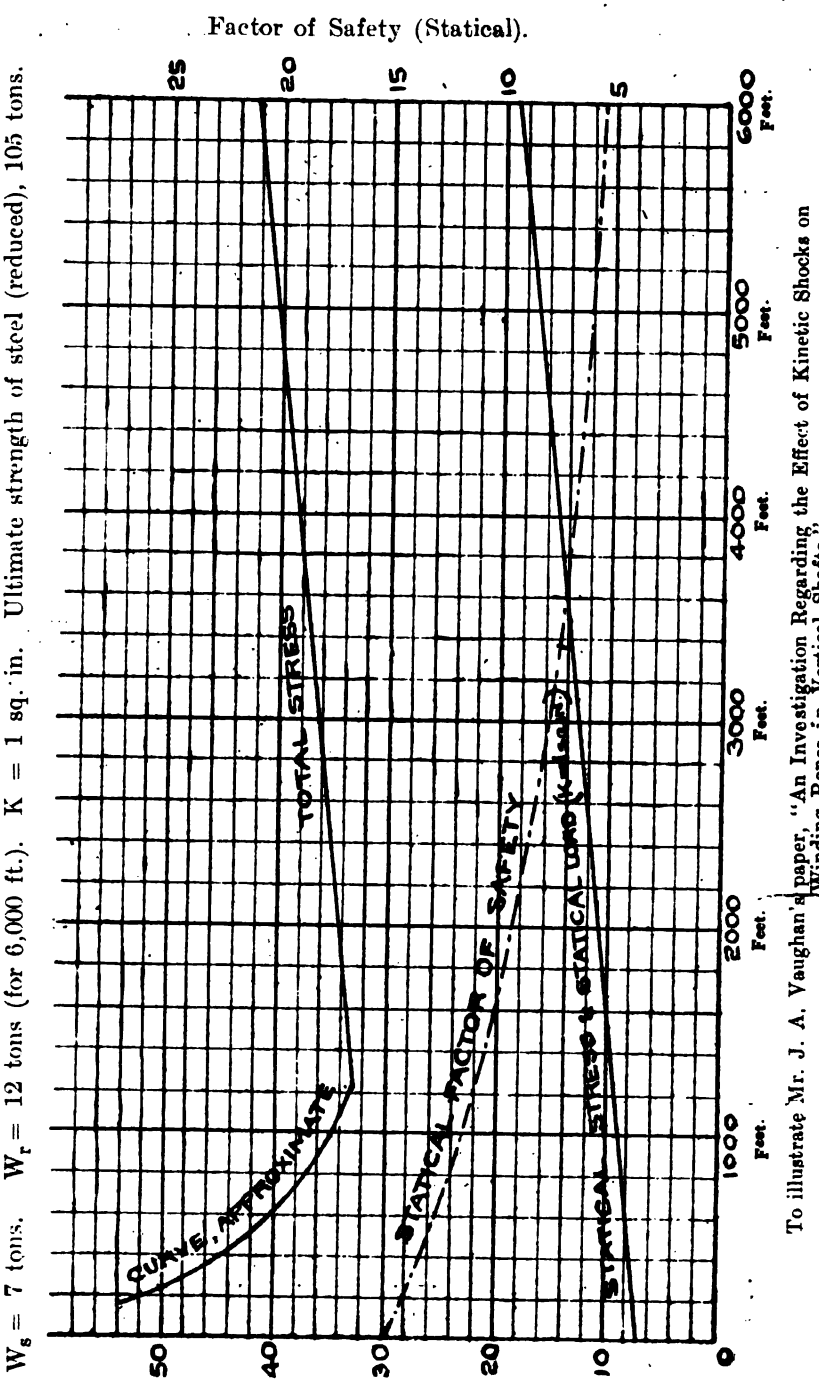


To illustrate Mr. J. A. Vaughan's paper, "An Investigation Regarding the Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts."

DIAGRAM C.

Showing Statical Stress and Factor of Safety at all stages of hoist of 6,000 ft., and Total Stress (Kinetic and Statical) at upper cross section due to sudden stop when lowering at 10 ft. per second.

W_s = 7 tons. W_r = 12 tons (for 6,000 ft.). K = 1 sq. in. Ultimate strength of steel (reduced), 105 tons.



To illustrate Mr. J. A. Vaughan's paper, "An Investigation Regarding the Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts."

Discussion was subsequently invited on Mr. J. A. Vaughan's paper on—

"AN INVESTIGATION REGARDING THE EFFECT ON KINETIC SHOCKS ON WINDING ROPES IN VERTICAL SHAFTS."

Mr. Martin Epton said he had had the pleasure of helping Mr. Vaughan with the experiments referred to in his paper, and he wished to say that he absolutely agreed with the results. The author had brought out an important matter, viz., that there is a dangerous point in almost every rope, due to the vibrations from the kinetic shocks. He wished to be allowed to show them some photographs of steel taken from a rope, magnified some 750 diameters; and those taken from the end of the rope that never had any stress at all show it as a good steel should look. The photographs were taken in England, and the gentleman who took them did not know what the rope had been doing nor anything about it. The other part of the steel was taken from a badly worn place in the rope, some 300ft. from the end, and the whole structure of the steel was altered. The expert who made the photographs said it appeared as if the steel had been heated and plunged into the water: presumably he got that idea from the experiments that had been made for the Alloys Research Committee for the Mechanical Engineers, and it certainly had that appearance. Unquestionably the end of this steel where photographed in the part that had been subject to the stresses was entirely different to the part that had not been subject to any stress at all. He did not think the effect of wear, or anything of the kind, on the outside, would affect the structure of the steel in that way, therefore they must look to something else. And he thought Mr. Vaughan's paper helped them to find out the cause in the alteration of the structure, and also helped them to put that dangerous point at such a place on the rope that the danger would be greatly minimised.

Mr. G. A. Denny thought it would be of still further interest if they were told in what respect the structure of the two samples differed from each other: was it a re-crystalisation or re-grouping of the constituents: or what was the nature of the alteration that had taken place?

Mr. J. A. Vaughan said they were fortunate in knowing that Professor Arnold, one of the leading men on this subject, was coming out with the British Association to South Africa this year. He had thought of sending him two specimens of a much-worn rope—not worn on the surface, but a rope in which the wires have deteriorated, and where the weekly examination showed they must be getting near their limit stage, there being many broken wires, such deterioration increasing in geometrical progression. The material had practically altered in composition, apparently, it seemed to him to have re-crystalised in some way. He intended when sending these specimens to ask Professor Arnold whether he could deal with the subject in any remarks he might make here.

Mr. C. B. Brodigan was of opinion that the change might arise by other than alternating processes. He knew a case where one end of a guy rope was embedded in ashes and it broke off suddenly, and he found that the wires could afterwards be cracked, whereas about three feet higher up the rope the individual wires still had a perfectly fibrous structure. Whether this was due to being embedded in the ashes was worthy of investigation.

Mr. Martin Epton: If you put pipe in ashes it will soon disappear owing to the sulphur in the coal.

The Chairman: In most cases here, with the coal we have, you would find no steel or iron would last long in contact with ashes in the open air.

Mr. C. B. Brodigan: It preserved its original section entirely: it was a full section of the rope.

Mr. M. Francke said the process they were using in making forged iron or steel out of cast iron was really reversed in the process of deterioration under vibration. If the fibres are subjected to vibration, the natural tendency of the material asserts itself and octahedral crystals are again formed, which, of course, are of considerably smaller dimensions and not elongated like the original fibres. He thought that was the explanation of the deterioration in steel or forged iron if subject to vibration. It had been observed in railway bridges, although not to such an extent as steel wires which are subject to vibration.

Mr. G. A. Denny: I should like to mention stamp stems. Iron naturally crystallises in octahedrons, which, in the process of forging and wire-drawing, are elongated into fibres.

"AN INVESTIGATION REGARDING THE EFFECT OF KINETIC SHOCKS IN WINDING ROPES IN VERTICAL SHAFTS."

By Mr. J. A. VAUGHAN.

AUTHOR'S REPLY TO DISCUSSION.

The author will hardly be expected to give anything more than a formal reply to the paper that was presented over twelve months ago, and which has elicited practically no criticism. For the kindly and appreciative remarks of some of the members the author tenders his best thanks. He cannot refrain though from expressing regret that the important subject dealt with has been so scantily discussed. It appeared impossible to present the matter in a very attractive form, but still the conclusions arrived at might have been either attacked or acquiesced in on general grounds by those who, for want of time or inclination, were unable to thoroughly digest the previous somewhat lengthy arguments.

Conclusion No. 4, viz.: "For all depths greater than the dangerous limit the maintenance of a standing reserve of strength in the rope appears a suitable provision against the effects of kinetic shock," is of the utmost importance, and the author feels that a decision in this matter must be sooner or later arrived at.

From the Journal of the Transvaal Institute of Mechanical Engineers.

VOL. II. PART 8, 1905.

THE RISK ARISING FROM USING WINDING ROPES WITH TOO HIGH AN INITIAL FACTOR OF SAFETY.

By Mr. H. J. S. HEATHER (*Member*).

In these notes the question of wear on ropes only is dealt with, and not that of corrosion, although it is fairly evident that starting from the corrosion point of view somewhat similar practical results will be arrived at.

According to the Government Regulations the Resident Engineer on these fields is required to throw out a winding rope when it has, in his opinion, deteriorated to such an amount that its factor of safety does not exceed six. Now, as the wear on the rope will in most instances not be uniform, he has to base his opinion on the known original strength of the rope's component wires, and his estimate of the extent to which their cross section is reduced by wear at the most worn part of the rope. We will assume that his estimate, formed without opening up the rope, will be, if he is a good man (personally I should say a *very* good man), correct within 10 per cent. Now, take two specific examples; suppose the original factor of safety, is 10, and the engineer estimates that 30 per cent. of the cross section is worn away, he believes he has a factor of safety of seven. If his estimate is 10 per cent. wrong, 40 per cent. of the section is worn away, and he has a factor of safety of six. That is, a 10 per cent. error reduces the factor of safety from seven to six.

Suppose on the other hand the original factor of safety was 20, and the engineer estimates that 65 per cent. of the cross section is worn away, he believes he has a factor of safety of seven. But if he is in this case 5 per cent. wrong and 70 per cent. is worn off, he has only a factor of safety of six. That is, a 5 per cent. error reduces the factor of safety from seven to six.

It is clear, therefore, that if the initial factor of safety is doubled, the result is to make it necessary to reduce to one-half the error of the observation from which the final factor of safety is deduced, or to put it another way, doubling the initial factor of safety, given equal accuracy of observation, doubles the chance of ultimately making a fatal error.

Next, the more a rope is worn the slower the rate of deterioration appears to be. Up to the point where half the diameter of the outside layer of wires is worn away it is comparatively easy to estimate the wear, but as soon as this stage is passed, a difficulty comes in. The flat worn surface of each wire should, theoretically, from this stage diminish, but in practice this cannot be expected to be so. The thin edges of the worn wire, instead of being worn off, will begin to burr over under the pressure of the drums and sheaves, and although far more

than half the section of the wire may be gone, the apparent diameter retains very closely its original value.

The consequence is, that though up to the stage where half the diameter of the outside wires is worn away, it is fairly easy to estimate the amount of wear, as soon as this stage is passed it becomes extremely difficult, the estimation depending upon a measurement of the diameter of the whole rope (always supposing the rope is not opened up, a proceeding which most practical men will not favour) and this means multiplying the chances of error by 10 or 12 in most cases.

The conclusion of the matter, therefore, from the wear point of view is this: That a rope should be discarded at the latest by the time half the diameter of the outside wires is worn away, which means, practically speaking, that in order to avoid accidents the initial factor of safety should not be so high as double the final one. If the final one is fixed by Government regulations at 6, the inital value should not be so high as 12.

This is a point that will be of interest to those concerned with deep level winding, where a heavy rope is usually out of the question.

The PRESIDENT (Mr. A. C. WHITTOME): Mr. Heather has dealt with an important subject, and as he says at the close of his paper, the deeper the shafts are going the more important it becomes. He states that he thinks a very good man might estimate the amount of wear on a rope within 10 per cent. I think he would be an exceptionally good man if he got within 10 per cent. in an estimate of the breaking value of some of the ropes here.

Mr. VAUGHAN: I presume Mr. Heather was referring solely to external wear when he mentioned 10 per cent.

Mr. HEATHER: I was referring only to the wear that could be observed from the outside.

Mr. BERRY: I do not think the liability to accident with winding ropes depends so much on the outside of the rope. A new rope after three or four weeks' running will show a lot of wear, and after that one cannot expect so much in the next three months. If the apparent wear during the first month continued at the same rate, the rope would have but a short life.

Mr. THOMAS: I agree with Mr. Berry to a great extent with regard to the actual wear of the wires in any rope. So far as my experience goes, ropes on these fields are more often taken off on account of broken wires than on account of the actual wear which has taken place. There are few, if any, long ropes that work on these fields which do not contain brazed wires. Under certain conditions of work these brazed wires break soon after the rope has been put to work, but I do not consider that there is much to be feared from this cause, as it is almost always possible to judge the amount which the breaking strain has been reduced by a broken braze. We know the size of the wire and the strain per square inch to which the wire has been drawn, and it is a simple matter to calculate the breaking strain of the wire. Speaking generally, this will be very nearly a reduction in the breaking strain of the rope. When the wires begin to break through some deterioration in the steel, which can be seen by a considerable number of wires breaking throughout the rope at unbrazed places, it is usually time to remove the rope. Sometimes it is possible to keep a rope on until two or more wires are broken together, or very close to each other. At times the wires of a rope wear down to such an extent that the factor of safety is reduced below 6 without a single wire in the rope breaking, in which case, of course, the rope would have to be removed. I do not think Mr. Vaughan will object if I say that I think his opinion is that with a rope longer than, say, 2,000 ft., the factor of safety could be very much less than with a shorter length. As the rope is wound up the factor of safety increases with the diminishing weight of the rope, and if Mr. Vaughan's theories are correct, I think that the factor of safety for great depths will very much be reduced in the future.

Mr. JOHNSON: There is one point in Mr. Heather's paper to which I should like to refer. I think Mr. Heather has been referring all the time to a rope of 6 round 1 construction. I should say his remarks, anyhow, would be modified considerably with a different construction of rope, for instance, with a composite rope of 9 round 3.

The PRESIDENT: It may interest the members to know that one of our members told me a little while ago that he would very soon be able to provide mechanical engineers with a small instrument which

would enable the engineer not only to determine the external wear on a rope, but the internal corrosion also.

Mr. VAUGHAN: I can back up to a certain extent what the President says, and I think there is a possibility of the success of the instrument. I believe it is possible that the sectional area of the wire remaining in the rope may be ascertained from indications given by the instrument. Of course, that will form no guide as to the number of broken wires inside the rope. In ropes of compound construction the wires may not only be broken, but they may be seriously dented into one another and worn in certain parts. In my experience of ropes which have broken, it has generally been the result of severe strain, due to such causes as overwinding, or jamming in the shaft. Generally, the breaking of the rope has been accountable, but there have been cases lately brought to my notice in which the deterioration of the rope has been apparently unaccountable. For instance, the breaking of wires which are very little worn on the surface at all, and that have subsequently shown very little wear underneath. The wire has broken in practically its original fulness, due apparently to its having deteriorated in quality—in fact, become brittle. I have referred this matter to Prof. Arnold, who is coming out here with the British Association. I do not know whether it will draw from him an explanation, but I hope it will, because he is an expert in the deterioration of steel under stress. I sent him several specimens of broken wires, taken from a rope which showed very little indication of surface wear at all. I do not think we shall be troubled a great deal in the future by ropes having too high a factor of safety. In deep level winding the inital factor of safety is sure to be reduced, say, to 7 or 8, and there will be the difficulty then of the engineer assessing the strength remaining in the rope. He will have to go into the matter very carefully. Regarding Mr. Thomas's remarks, I *did* write a paper trying to show that after a certain depth was reached—roughly, when the weight of the rope was equal to a third of the load it carried—a certain position of comparative safety was reached, and proceeding lower after that point a reserve of strength was all that was necessary. I am now in the unfortunate position that my views have not been either traversed or corroborated. If corroboration is forthcoming, we may possibly look forward to a reduction in the factor of safety at great depths, in the provision of a reserve of strength, and not necessarily a multiple of the load.

Mr. W. H. SHAW: I had rather a peculiar experience some two or three months back. It was found that with a rope that had been in use for about two years the wear was fairly equal, but one morning, about an hour after inspection, six wires were found to be broken; strange to say, at a part that was not worn quite so much as the main part of the rope. When a rope is taken off or turned round, I think it would be advisable to open out the strands and bend them to see what has actually taken place. The rope which I have referred to had an original breaking strain of 58 tons, and when tested at the Government laboratory it showed a loss of only 2 tons.

Mr. THURSTON: I think it is very easily explained why Mr. Vaughan's paper has not been criticised to the extent it deserved. When Mr. Vaughan wrote the paper he took great pains with it, and to disprove the statements therein involves an amount of time that very few people can devote. The paper is an excellent one, and I am perfectly in accord with the author's theories with regard to long ropes, especially after I had seen the methods of arriving at them. I think Mr. Berry is also right in his remark that a new rope shows the greatest amount of wear during the first month. This may be accounted for by the fact that the amount of wearing surface during the first month is smaller than after the rope has been in use for some time. There is another factor which I think enters into the life of a rope, and that is the changing of the nature of the material. We all know that acid water in a mine has such a deteriorating effect on cast-iron or steel that in the course of time it is possible to cut either of these metals with a knife; how the instrument to which Mr. Whittome referred will take into consideration this change I am curious to know. The thorough examination of a rope is a proceeding requiring much more time than as a rule is given to the resident engineer for this purpose. In fact, it would mean the suspension of hauling for two or three hours daily, which is absolutely out of the question. It is

not to be expected that the resident engineer is a rope expert. Other duties in connection with his calling leave him but scant time to devote to the study of ropes, consequently it is only after long practice that his judgment becomes of value. Moreover, at the present time an engineer has but little to guide him, and recent events have a tendency to make him conservative and condemn ropes that would in the ordinary course of events last considerably longer. The personal element also enters largely into the examination of ropes. As an instance, six engineers may examine a rope, five of whom may say that the rope is fit for use, while the other may say it is not. From this it would only be fair to assume that the rope could be used, yet if it was to fail shortly after, the judgment of the majority would seem to be at fault. Cases of this kind are not impossible in practice. Ropes have been known to fail shortly after they were pronounced by experts as fit for use. I think if some regular and accepted method of examining a rope could be devised, in order that the resident engineer could act on this method and say that according to the accepted method the rope should or should not be used any more, it would be the best conclusion that could be arrived at.

Mr. VAUGHAN: Regarding that rope I spoke about just now, just to show members how things go, I may say it was put on on the 9th October, 1904. It was shortened on the 20th October, and on the 4th December there were four broken wires. The rope was very carefully examined. I think the rope passed through the hands of eight kaffirs, as well as being watched by the manager, engineer and rigger. On the 1st January four more wires were broken, on the 8th five, on the 15th fourteen, on the 22nd twenty-two, on the 29th twenty, and on the 5th February twenty-nine. Of course, it is just possible that the same wire may appear as the broken wire many times, but, as far as breaks in the wires go, there were, roughly, 100 breaks. I do not know how many more it will show at the next examination, but large increases may be expected. That rope is very little worn on the outside at all, and it becomes a ticklish duty for the engineer to assess the strength remaining in the rope.

Mr. THURSTON: I should like to know the size of the pulleys on which the rope is working, and whether there are any reverse bends on the rope. As we all know, the diameter of the wire compared with the diameter of the pulley has a very great influence on the life of the rope.

Mr. VAUGHAN: As regards the size of the pulleys the rope is pretty well off, but there are one or two reverse bends.

Mr. BERRY: Another important factor comes in. Has there been any change of engine drivers?

Mr. JOHNSON: I think it is a very interesting case that Mr. Vaughan has just cited, and I should like to ask Mr. Vaughan if he can tell us if it is entirely due to the rope that all these wires broke, or is it due to the engine?

Mr. VAUGHAN: I think, gentlemen, I have said enough.

Mr. THURSTON: I have noticed that one of the worst features is where there is a small drum containing a number of coils, when the rope mounts up on a fresh coil a shearing action takes place, which never fails to break the wires.

Mr. THOMAS: I agree with Mr. Thurston. I have seen ropes in vertical work in which the portion of the rope on the drum is the only portion enough worn to cause the removal of the rope. This portion is the under layer, and part of it would never come off the drum. The conditions under which this might happen are when loads are hoisted at high speeds and the ropes coil one layer over the other on the drum. Mr. Vaughan says that he has not had any corroboration of his theory in regard to the factor of safety in deep levels. In regard to this I should like to give a few facts which have come to my notice from time to time. In one case, in a sinking shaft, the bucket swung and pulled out two wall-plates. The rope in this case was a $\frac{3}{4}$-in. diameter plough steel rope, with a breaking strain of 27·2 tons, and the factor of safety at the top of the rope was 9·4 tons. In another instance the bucket caught and the bridle was sprung, allowing the bucket to fall into the bottom. In this case also the rope was a $\frac{3}{4}$-in. diameter plough steel. In a third instance, with the same size of rope, the bridle of the bucket was broken. On investigating the condition of the iron of which the bridle was composed, it was found to be slightly crystalised, but the

strain necessary to break it would have been great. These things all happened below a depth of 2,000 ft. It might be possible in every instance that the strain on the ropes was not as great as it appeared. These cases, to my mind, all tend to confirm Mr. Vaughan's theories.

Mr. LEUPOLD: Reference has been made to 6 over 1 strand composing the rope. I would like to mention that the central core wire of the strand is subject to a greater stress than the other ones. Prof. Hrabák, in his well-known book, has shown that if the modulus of elasticity of a given wire be 1·00, then the modulus of elasticity of said wire laid in strand is ·60, and when laid in a rope is ·36, therefore the central core wire in a rope would be subject to a stress nearly double that of the other wires, therefore in calculating the strength of a rope the central wire of each strand should really not be considered at all, because it is subject to so much more stress. The practice, I believe, with nearly all makers is to make the central core wire of much milder steel, so that it relieves itself of the stress.

MR. THURSTON: I believe that some rope makers do not take the central wire into consideration at all.

MR. THOMAS: If the centre wires in the strands of a rope are of low strain material, the breaking strain of the rope will be lower than if these wires are of the same strain as the outer wires. I think most of the rope makers agree that, under ordinary conditions of work, a rope gives better results with a soft centre wire in the core of the strands than with a hard centre. If it be necessary for the rope to have a very high breaking strain, the hard centre can be used. In taking a rope apart, I never found that a hard centre wire has deteriorated.

MR. THURSTON: I have known ropes in use in which the centre wire was broken.

MR. THOMAS: In calculating the actual breaking strain of a rope, I do not usually take into consideration the core wire, as I always deduct a certain percentage for loss in spinning. Among the different makers the methods of calculating the breaking strain of a rope varies. Some makers use the aggregate strength of the wires as the breaking strain of the rope, in which case, of course, a soft core wire should not be considered. Other makers deduct a certain percentage from the aggregate strength of the wires. By this method—if the core wires be soft and the total number of wires, including the core wires, be used for finding the aggregate strength of the wires—the percentage to be deducted for spinning would be greater than if the core wires be of high strain. These percentages can only be found by having ropes tested with both hard and soft core wires. In calculating the actual breaking strain of a rope, I always multiply the average breaking strain of the wires by the total number, and deducting the larger percentage.

MR. LEUPOLD: I meant to say that according to the latest scientific researches it has been found that the core wire in the strand is subject to a greater stress than the outer wires of that strand, therefore the strength of this central wire cannot be counted on in the same degree as that of the outer ones. If any wire broke through stress, it would be the inner wire that would break first. That is not what happens generally, but this is owing to the fact that the outer wires are worn down to a fraction of their original diameter, whereas the inner wire is not worn down so.

MR. JOHNSON: If you have a 7-strand rope you must not allow your wires to wear more than half, but if you have 8 round 4, the amount of section which is worn away is very much less after you have got the outside wires worn down to a half. That is to say, you have still got four good wires inside.

MR. THURSTON: Mr. Johnson might endorse my remarks a little further by suggesting that the Government should take this matter up. There is plenty of room for them to decide what method shall be used for determining the factor of safety. When a rope is purchased it is taken on the manufacturer's guarantee with regard to the breaking strain. Whether the centre wire is to be of value is a thing that should be settled by the Government. I agree with Mr. Leupold's remarks regarding the stress on the core wire. Prof. Hrábak has brought the matter into great prominence. I might draw a comparison with the Lambeth cotton rope. Mr. Hart, who is an acknowledged authority on cotton ropes, will tell you that the inner coil of his rope is composed of strands which are not twisted very much. These strands take by far the great-

est strain that comes on the rope. The outside strands, which are used as friction bands and have considerably more twist, are not taken into consideration with regard to the strength of the rope. I am very strongly of opinion with regard to ropes that there should be certain definite rules laid down, so that when we are buying ropes it is not altogether left to the maker, who naturally wishes to sell a rope having the highest breaking strain at the lowest price.

MR. VAUGHAN: When the Committee on Standardisation met in this room we got through a certain programme presented to us, and as it seemed to me rather a pressing need that there should be a standard specification for a wire rope, I proposed its formation. A committee has now been elected which will attempt to draw up a standard specification for wire ropes. As a matter of fact, my experience is that the majority of makers disregard the central wire, or at any rate its value is altogether eliminated by the allowance made for spinning. Cases have occurred in which this central wire has been broken. Then it proved fairly hard steel. Whether it was originally hard, or had hardened with continual use, I am unable to say.

MR. HEATHER: First of all I must apologise for bringing up a paper that I did not originally intend should come up for discussion here. Mr. Berry, and I think Mr. Thurston also, mentioned the fact, that ordinary wire ropes appear to wear much more quickly at first. This corroborates what I said, that the more a rope is worn the slower the deterioration appears to be. In regard to wires breaking, Mr. Thomas referred to this as determining the life of a rope more often than the wear. That depends, it seems to me, altogether on the rope in question. I have seen wire ropes where the section of the outside wires have been worn down to a quarter of the original section without breaking. I have also seen wires, that have never been round a drum, broken without any signs of wear, quite away from a braze. I think that is a question of the temper of the steel that it is made of. With regard to the construction of the rope, I think that is a point which certainly ought to be enlarged upon, whether it is 9 over 3 or 6 over 1. I quite agree with Mr. Leupold when he says that the core wire of a 6 over 1 rope ought not to be considered as contributing at all to the strength of the rope. I may mention that this is not confined to wire ropes. I have seen a Manilla rope in which there was not a continuous length of 18 ins. where the core rope was not broken. I think that would show pretty well that you need not consider the core even of a wire rope with regard to judging the strain. Mr. Vaughan mentioned that in deep level winding we shall probably come down to a factor of 7 or 8, and if I understood him right he said that would make the engineer's work more difficult. I entirely disagree with him. I consider that if you start with a factor of safety of 7 or 8, and stop when you get down to 6, it would be very much easier to determine that point than if you start with double the factor of safety. I contend that the lower you put the factor of safety, the easier it will be for the engineer to determine when the external wear has gone far enough. Mr. Thurston raised the question of the proper determination of the time when the rope is finished. It seems to me that the only really valid way of determining whether a rope has a factor of safety of 6 is to put six times its working load on to it and see if it breaks, and I would suggest that that be done every month.

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WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

The ropes dealt with in this paper are those in use on the Transvaal, and the working conditions referred to are the local conditions that exist on either our Gold or Coal Mines.

It is not intended to describe the methods of manufacture, these, together with drawings and descriptions of rope-making machines, may be studied in recently published books, such as Hrabak's "Die Drahtseile," or Bucknall Smith's "Wire: Its Manufacture and Uses."

The intention of the writers is rather to make public the results of experience gained in the examination and testing of wire ropes in the Mines Department Mechanical Laboratory, and, as bearing directly on the subject of deterioration, the writers present the views of several authorities on such matters as corrosion, galvanic action, and the action of nascent hydrogen on steel.

The operation of winding is probably the most important of all mining operations. The output of the mine is dependent upon it in a double sense; not only does it supply the means by which the miners travel to and from their work, but it also hoists the ore, the result of their labours, as well as in many cases dealing with the mine drainage.

The wire rope may perhaps be regarded as the most important item of the winding plant. Daily and monthly examinations are enforced by law, and, considering the serious nature of a failure in the rope, it is hardly possible to devote too much attention to the matters affecting its condition.

The duration of the working life of a winding rope depends to a great extent on its suitability for the work it is called on to perform. A rope of a certain grade and construction, which is specially adapted for work in one particular winding proposition, may not be successful, in fact may be a failure, if put to work in another shaft. While the duties of different ropes employed under identically the same conditions are capable of comparison, the duty of one particular rope in comparison with that of another rope working under different conditions is a difficult matter to assess. In the former case, the depths of working and all other circumstances being the same, the simple formula

$$\frac{\text{Total ore hoisted}}{\text{Cost of rope}}$$

would apply, but in the general case it appears to the writers that the correct estimate would be more nearly approached by a formula such as

$$\frac{\text{Mileage run}}{\text{Factor of safety}}$$

it being necessary, however, to consider the true effective strength of the rope in comparison with the total effect of *all* the destructive influences, and not merely the statical load, in estimating the real factor of safety of the rope.

The cost of different classes of ropes to obtain the same factor of safety is very probably about the same.

Until the probable magnitude of the influences producing stress and deterioration in the rope can be calculated, so long shall we continue, as at the present day, to create, by means of an arbitrarily fixed multiplier, a barrier against accident, which, as we all know, does not in every case prove effective, and which, even while it may be safe, may not be economical.

Further, when the provision of this factor of safety appears lavish in its protection, it may for this very reason inspire a confidence leading to carelessness.

The present contribution, together with another paper presented by one of the writers to the South African Association of Engineers dealing with the effects of kinetic shocks* on winding ropes, will, it is trusted, serve to point attention to, and promote discussion on, this important subject, and it may not be too rash to hope for some advancement beyond present practice.

Steel, as it appears in our winding ropes to-day, is of far higher tensile strength than that used a few years ago, and while it is true that high tensile efficiency is obtained at the sacrifice of toughness and ductility, the extent of the sacrifice is not as great as many people suppose.

This steel is manufactured from "blister or cemented steel" by the crucible process, with the addition of carbon and manganese. The percentage of carbon in the product varies in different qualities from 0.5 to about 0.8, and the percentage of manganese from 0.3 to 0.7. The percentage of silicon is kept as low as possible, say 0.15, while phosphorus and sulphur are only tolerated in the most minute quantities, their presence being fatal to the success of the drawing, annealing, tempering, and patenting processes that produce high grade wire.

The tensile strength met with in wire ropes on these fields at the present time

* "An Investigation regarding the Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts." A paper read before the South African Association of Engineers, by J. A. Vaughan, on 30th March, 1904. [See pages 67—83.—Ed.]

varies from 90 tons* to 160 tons per square inch, and the meaning of the trade terms describing the varying qualities is apparently as follows:—

"Improved Plough" from 130 tons per square inch upward.
"Best Plough" .. from 115 to 130 tons.
"Plough" .. .. from 100 to 115 tons.
"Improved Patent Crucible" about 95 tons.
"Best Patent Crucible" about 90 tons.

It is provided in the Mining Regulations of the Transvaal that the breaking stress of the steel of which the rope is made shall be stated in the manufacturer's certificate, so that no ambiguity or uncertainty exists on these fields, owing to the use of trade terms, as to the grade of steel guaranteed to be supplied in any rope, so far as tensible strength is concerned.

The following tables, taken from Hrabak's "Die Drahtseile," show how the flexibility of high grade steel wire has increased in recent years owing to improvements in the methods of manufacture. The length of wire subjected to the torsion test was 200 mm. (=7.874in.), and the radius of the 180° bends in the bending test was 5 mm. (=·19685in.)

Hrabak states that while in earlier times the modulus of elasticity of good iron wire amounted to about 16,000 kg. per mm.^{2} (=22,757,000 lbs. per sq. in.) and while that for hard steel came high above 20,000 kg. per mm.^{2} (=28,446,000 lbs. per sq. in.), the last amount has scarcely ever been reached with the modern hardest drawn steel wire used for winding ropes (128 tons per sq. in.).

At the present time in the province of Anhalt, Germany, the Mining Regulations prescribe the following minimum degrees of flexibility for the wires composing the rope; that is to say, wires taken from the lower end of the rope at the time of re-capping (every three months) must be able to stand:—

Wires up to 2·0 mm. diameter (=·0787in.), 8 bends of 180° before breaking.

Wires from 2·0 to 2·2 mm. diameter (=·0787in. to ·0866in.), 7 bends.

Wires from 2·2 to 2·5 mm. diameter (=·0866in.), 6 bends.

Wires from 2·5 to 2·8 mm. diameter (=·0984in. to ·110in.), 5 bends.

EXPERIMENTS CARRIED OUT SEVERAL YEARS AGO.

Ultimate tensile stress of material. Tons of 2000 lb.	42·67			85·34			106·67			128·01		
Diameter of Wires (in.)	·055	·078	·098	·055	·078	·098	·055	·078	·098	·055	·078	·098
Number of 180° bends	27	18	14	26	17	12	26	16	11	25	16	10
Number of rotations	60	52	40	26	22	18	26	20	17	25	19	16

EXPERIMENTS CARRIED OUT IN 1900.

Ultimate tensile stress of material. Tons of 2,000 lbs.	106·67			
Diameter of Wires (inch) ...	·043	0·47	0·55	0·63
Number of 180° bends	63	36	30	23
Number of rotations	50	42	40	34

* All tons mentioned in this paper are "short" tons of 2,000 lbs each, except where otherwise stated

Wires from 2·8 mm. diameter upwards (·110in. upwards), 4 bends.

Regarding the relation that exists betwen the flexibility and tenacity in high grade wire, the remarks of Mr. Kenneth Austin* may be quoted:—

"The demand for higher tensile strength for steel wire could be met, at present, only with an accompanying loss of ductility; from inquiry he had found that wire makers would not guarantee an increase of tensile strength above 120 tons per sq. in., without a corresponding loss in ductility of about one twist less for each ton added to the tensile strength of the rope. One of the tests for ductility was to demand a certain number of continuous twists, before breaking, in a wire which had a length of 100 times its own diameter. The following were some of the standards:—

Breaking strain per sq. in. Tons.	Twist before breaking.
90	38
110	36
120	32

"The tests for ductility were usually carried out by a machine 8in. between the grips holding the piece of wire; this length of 8in. was arrived at by multiplying 14 W-G (=·080in. by 100=8in.; all torsional tests being proportional and related to this standard."

He goes on to say that:—"There was generally too little consideration given to the torsional test, and too much to tensile strength; ductility was of the greatest importance, for the wire ropes when in use were turning round the whole time they were at work, in consequence of their spiral formation, and it was reasonable to assume were subjected to severe torsional strain when they were submitted to shocks."

Writing on this latter point, Hrabak states that the torsion test appears to him to be of less importance than the bending test, as the wires of the rope, while being laid up in the process of the manufacture of the rope, or during its use, are not subjected to torsion to any extent. The test, however, is adopted to furnish testimony as to the homogeneity of the material.

Table I. shows the results obtained by the writers in the Mines Department Mechanical Laboratory in tests of wires of various sizes taken from new ropes. The ultimate tensile stress varied from 82·6 tons to 159·8 tons per square inch. The number of bendings (180°) over ¼ inch radius obtained varied from 4·9, in the case of a crucible steel wire of ·128″ diameter, to 50·7″ in the case of a best plough wire of £051″ diameter. To compare the number of bendings in different sized wires of different grade steels is a difficult matter. The great difficulty lies in the fact that the breaking stress of the material decreases as the number of applications increases, and also it has to be remembered that the stress produced even at the first bending exceeds the elastic limit of the steel, judging the latter by the tension test. The elasticity of the skin of the wire may, however, be different from that of the remainder of the section.

Regarding the test from any standpoint $\frac{\text{ultimate stress}}{\text{number of bends}}$ does not give a correct basis of comparison. The writers have taken the *square root of the number of bends* in a formula

$$\frac{\text{ultimate tensile stress of steel} \times \text{diameter bent over}}{\sqrt{\text{number of bendings}} \times \text{diameter of wire}}$$

which would give perhaps a number proportional to E_0, the modus of elasticity of the wire. Anyway, the approximation gives very interesting results. Column 7 in Table I. shows this comparison.

In the torsion test, the number of rotations depends on the quality and size (diameter) of the wire under test. It has, however, also to be considered how evenly, or otherwise, the spiral lines produced by the rotations are spaced along the length of the wire. This effect bears witness to the homogeneity of the steel, and can be observed by the feel, or by the appearance, in the latter case either with or without magnification.

To compare the number of complete torsions obtained from wires of various sizes and grades, the writers have, for want of any better formula, used the following:—

$$\text{No. of complete turns} \times \text{diameter of wire} \times \frac{\text{ultimate tensile stress of steel}}{\text{length of test piece (inches)}}$$

the result being shown in column 9 of Table I.

How far either of the above-mentioned comparisons is accurate will no doubt be

*Transactions. Institute of Mining and Metallurgy. Vol. XI. Part 1, page 248.

commented on in the course of the discussion on this paper.

With reference to the length of the specimen in torsion tests, which, as previously stated, is generally 8 inches, the writers made a few experiments with other lengths. The results they obtained seemed to show strangely enough that the length of 8 inches was especially favourable, for the torsions per inch length decreased, both in lengths shorter and in lengths greater. This refers to effects noticed in wires of from ·081″ to ·128″ diameter.

Winding ropes in the Transvaal are in nearly every case composed of six strands around a hemp core, and, with few exceptions, have the component wires of the strands twisted in the same direction as the strands are twisted in the rope, *i.e.*, Lang's Lay. The lay is practically always right handed. On this point Mr. T. C. Futers* may be quoted:—

"Lang's Lay ropes may be twisted either right-handed or left-handed, and they should be made according to the direction they coil on the drum, *i.e.*, if, when standing behind the drum and looking towards the pulleys, the rope coils from right to left, it should be laid left-handed, and *vice-versa.*"

As the writers state, no such provision is made locally as a rule, and it is doubtful whether any extra wear is occasioned in the rope by the neglect of this point. The custom on the Rand is to secure the ropes on the outer sides of the drums, presumably for convenience sake. Where the lead of the rope, from pithead sheave to drum, is very oblique, and there is a tendency of the rope to "climb" (*i.e.*, to mount on itself) on the drum, this tendency might possibly be counteracted by using a left-hand lay rope on that particular drum.

There exists a great variety of constructions of the strands of the rope, but in practically all cases the core is wire—generally a single wire. This construction Hrabak unequivocally condemns. He favours a hemp core in the strand; he styles the strand with the wire core a "stiff" strand, and rates it very high in the matter of bending stress, in fact, in the same grade as the hemp cored strands of a flat rope. Hrabak proves theoretically that the centre wires of cores carry 62½% of the full load so long as they do not break or remain immovable. He then goes on to show that the reason they do not break is that they remain gripped and in compression by the outer wires.

The only experience of the writers with hemp cores in strands of winding ropes was in the case of a flat rope. The strands in this case did not keep their shape well, and it may be anticipated that this disadvantage would also appear in the strands of a round rope, in which case the wires, by opening out, will allow the hemp core to get soaked with the mine water and thus tend towards deterioration by corrosion.

The number of wires composing the strands of round ropes in general use here (1in., 1⅛in., and a few 1¼in. diameter) varies from 7 to 22, made up in the following manner:—

6	round	1		...	Wires all the same size.
7	round	5	round	1	Wires differ in size, outside circle composed of larger wires and often of a little higher grade material.
8	,,	3	,,	1	
8	,,	5	,,	1	
8	,,	6	,,	1	
8	,,	7	,,	1	
9	,,	6	,,	1	All wires practically the same size, tending to have outside wires slightly larger, say ·003 in. difference.
10	,,	5	,,	1	
11	,,	6	,,	1	
12	,,	6	,,	1	

8 round a flat wire core of same grade as outer wires.
9 round 3 twisted in opposite direction to outside wires.
9 round 12 round a triangular core.

There are only three flat rope winding engines in operation on these fields now, one of which will soon be discarded, and the writers are therefore not noticing flat ropes specially.

The Mining Regulations prescribe that the breaking load (as certified by the manufacturer) of every new rope shall be entered in the Rope Record Book. For these certificates to have any exact value, there should be, among rope manufacturers, absolute uniformity in the methods governing their assessments. Such uniformity certainly does not exist on these mining

* "The Mechanical Engineering of Collieries." Colliery Guardian, 16/9/04.

fields, and apparently in British colliery work the same difficulty is encountered.

Mr. Futers, writing recently in the *Colliery Guardian*, states:—

"Considering the importance of the work depending upon the winding rope, it seems a pity that some standardisation has not been adopted. Manufacturers issue lists, some fairly complete, others very far from complete, and the breaking strains are usually given under such terms as 'plough steel,' 'patent steel,' 'mild steel,' and 'iron,' but the quality of the steel—as 'plough steel,' for instance—of one maker may be of very much higher tensile strength than another; and again, some makers give the aggregate breaking strength of the wires, as the breaking strength of the rope, which is not the case, the latter varying from 10 to 30 per cent. less than the former. It would appear, then, that such terms as 'plough steel,' etc., should be made obsolete, and the rope listed as manufactured from steel, with a certain tensile strength of so many tons per square inch, depending upon the quality of the steel used, and the ultimate strength of the rope should be given, which will be somewhat *less* than the total ultimate strength of the wires."

The writers certainly favour a standard specification for winding rope—the tensile strength and ductility to be stated in some uniform and exact manner.

Dealing now directly with the subject of the relation that exists between the strength of the whole rope and the aggregate strength of the wires composing it, the following quotations are given from Bucknall Smith's treatise on "Wire, its Manufacture, and Uses," page 209:—

"Mr. Biggart found that the strength of the component wires, individually considered, was about 10 per cent. above that obtained when in the form of roping."

From John A. Roebling's, Sons & Co.'s Catalogue:—

"The *strength* of wire ropes depends primarily upon the material of which the wires are made. It is hard to obtain from a sample of wire rope more than 90 per cent. of the aggregate strength of all of its wires in a testing machine, the average being about 82½ per cent. This is due to the difficulty in making perfect attachments to the ends of the test piece, in order that every wire shall carry its share of the load; to the fact that the inside wires of the strands are shorter than the outside wires and tend to break before them, and to the construction of the rope, which causes the adjacent strands to nick each other. While the last mentioned action reduces the breaking strain of a rope, it occurs only at stresses far above those employed in practice. On account of the nicking, ropes with a short twist or ropes made of hard steel break at a lower percentage of the aggregate strength of their wires than ropes with a long twist or ropes made of soft wire. So that while it may be advisable in certain cases to use a rope with a short twist, its breaking strain will be lower than if the twist were long."

The writers have carefully examined a large number of new ropes broken under test, and have failed altogether to detect any sign of this "nicking" or crushing effect of one wire upon another. These ropes were all Lang lay. The appearance of the broken ends of the wires was practically the same, in the matters of local elongation and reduction of section, as in the case of a straight wire broken under tension test.

Table II. shows the results of tests of new ropes in comparison with the tests of their component wires. The 29 tests appearing first in the table were made in the Mines Department Mechanical Laboratory from samples supplied by nine different manufacturers. The 11 following tests were made in England at two different well-known establishments. The length of the test specimens in the latter cases was 100 inches, which is about four times the length of the specimens in the other tests.

It has been argued by some that tests conducted on short lengths of rope, sealed at each end, are likely to give lower results in breaking load than might be obtained if the test specimens were much longer and if the individual wires were free to move and accommodate themselves to the rising strain. The writers have not found this view justified in the results they obtained, which are in practical agreement with those arrived at in other test houses with specimens of longer length.

With reference to the preparation of test specimens, it may be stated that the sample supplied, whether old or new rope, is always "served" at the ends, and, if it requires to be further shortened, new serving is always applied, so that the sample prepared

for test remains under its original strain, and no untwisting of the strands or wires is permitted. The test specimens are cut so that about a 5 in. free end projects beyond the " serving," and, in the preparation for test, the wires in these short ends are opened out, cleaned, and tinned. The ends are then placed between split taper moulds, and a white metal seal is run on. See Fig. 1, Plate I. When this metal sets each end of the specimen is held in a conical seal of a taper suitable to that of the gripping dies of the testing machine, and the lay of the wires of the part of the rope under test is in no way disarranged. The melting-point of the white metal is about 330 degrees Fahrenheit, and this heat does not lead to any annealing of the test specimens. As witness of this fact, the writers would point out that all test specimens break quite clear of their fastenings. The length of the pieces tested in the 50-ton (2,240 lbs.) Buckton machine is about two feet, clear of the seals.

Strands of ropes are prepared for test in a similar manner, the conical seals on their ends being smaller and suitable for smaller dies.

The single wires are tested for tensile strength in a separate machine of more delicate graduation. The ends of the wires are held, without any preparation, in self-gripping long-taper dies. Here, again, specimens usually break quite clear of their fastenings, and any test not so complying is considered useless. The torsion and bending tests for wires are of the usual standard type.

From Table II it will be seen that the tension tests conducted by the writers show a drop in efficiency for the wire " laid up " in the rope of from 3·65 per cent. to 17·4 per cent.

In considering the question of how far these results may be taken as applicable to much longer specimens, or to a long rope under tensile stress, it should first be remarked that the ends of each specimen are free to revolve, and do actually revolve as they require to, after overcoming the friction of certain rubbing surfaces. In the case of a specimen of new rope the alteration in angularity between the ends up to the ultimate load has amounted to over 90 degrees, while in an old rope the movement has usually been very slight.

The untwisting noticed by the writers would, if multiplied, in a 1,000 ft. new rope loaded up to the breaking strain, amount to about 125 turns, and corresponds to a certain extent with the " spinning " action of a new rope noticed in practice. A complete comparison of the elongation and the untwisting of the specimen might perhaps prove conclusively the correctness of the test, but the writers do not wish to lead this argument any further, as they feel convinced, as previously stated, that the test of a short specimen of the whole rope, if carefully carried out, is a fair test.

The writers freely admit that the results of their tests up to the present are not conclusive as to the deductions that should be made on account of " laid-up " efficiency in ropes of different sizes and of different constructions. They merely state at present that a general 10 per cent. deduction appears reasonable.

Table III shows the results of some tests carried out in the State University Laboratory, California, on ropes of varying grade of about $\frac{3}{8}$ in. diameter. Here may be seen a drop in efficiency of about 10 per cent. The length of the specimens tested in these cases was 12 inches.

A 200-ton (2,240 lbs.) Buckton Horizontal Testing Machine is now on order for the Mines Department Mechanical Laboratory. This machine will test specimens in tension up to 20 ft. in length, and possibly in the future some more definite information will be obtained regarding the actual tensile strength of ropes of all classes of construction and of higher breaking loads than those now reported upon.

The deterioration of a winding rope may take either any one or all of the three following forms:—

(1) Decrease in the sectional area of the wires;

(2) " Fatigue " or change of state of the steel, *i.e.*, loss of certain desirable qualities, such as ductility, originally possessed by the material;

(3) Discontinuity of the wires due to their breaking.

Regarding (1) this may be caused by frictional wear or corrosion, and it may not be confined only to the outer surfaces of the rope and strands.

Frictional wear is of course a necessary corollary to the working life of the rope. External friction is caused by contact with the drum and with other layers of rope on

the drum, by contact with the pithead sheave, and by contact with any other guiding or diverting sheaves or rollers met with in the trip.

Circumstances that increase its external wear are the following:—

Sheaves or guide rollers too heavy, their inertia causing chafing of the rope at every alteration of speed;

Winding of the rope on the drum in several layers, especially so if badly coiled;

Jerky running of the winding engine, *e.g.*, stopping or starting too rapidly;

Guide rollers or sheaves not working freely, worn unequally, or out of alignment;

Insufficient number of guide rollers in an incline or compound shaft, allowing the rope to foul the foot or hanging wall;

Flapping of the rope in a vertical shaft;

Angular friction caused by lateral deviation of rope, or obliquity of direction of rope, between pithead sheave and drum;

Bad bearing in tread of sheave.

Referring to the last-mentioned cause, it is very necessary in the case of a new rope of altered size or greatly different construction to the rope preceding it, that the tread of the sheave should be turned up to suit the new rope. The same course should also be followed if the sheave is changed.

Continual lubrication of the winding rope would, of course, diminish the wear, but the expense of the undertaking is sufficient to prevent such practice being adopted, apart from the practical difficulties connected with the step. It may be noted, however, that haulage ropes are in some cases kept well greased by their running over a sheave pulley working in a box containing lubricant for the purpose, and the life of the rope is increased.

On these fields it is the custom to treat the winding rope, about once a week, with a composition which is supposed not only to lubricate the rope, but also to preserve it from corrosive action. Table IV. shows particulars regarding the compositions in use at nine mines. The best dressing for the rope would be one of neutral, or possibly alkaline, reaction, and of such consistency that it will not only remain on the rope under all working conditions, but will also penetrate to the interior.

From Table IV. it will be seen that the compositions used are not in every case the best possible.

Internal frictional wear is caused by the pressure and rubbing of one wire against another, either in the same strand or in an adjacent one. Every alteration of stress in a winding rope tends to alter the spirality of the strands in the rope, and that of the wires in the strand, so that, in ordinary working there must always be friction between adjoining wires. This action may be more severe, and its effects more marked, on account of the following causes:

The want of internal lubricant;

Badly trimmed joints of the wire. Brazed joints should be filed or ground smooth and reduced down to the same size as the wire;

Bending of the rope round a curve of small radius;

The core wires of strands being of high grade steel, or of special section with cutting edges such as △.

The two latter effects are more severe in ropes with long " lays " of wires or strands, as also in some ropes of compound construction in which the wires are not of the same grade steel.

Regarding the matter of internal lubrication, it is the general custom in the manufacture of the rope to soak the hemp core with some lubricating composition, but there appears a great deal of difference between the ropes of different manufacturers in this respect, in the matter of the thoroughness of the greasing.

In some cases, owing to the process of manufacture, the hemp core provides practically a reservoir of lubricant, while in others it may be likened to a dry sponge ever ready to absorb moisture. When a new rope is put on it should be thoroughly treated with the composition before the mine water gets a chance to wet it. This course may not be absolutely necessary in all cases owing to the manufacturer's foresight, but it is a prudent course to adopt.

In Mr. Biggart's experiments it was found that in the working life of ropes, under certain conditions, the destruction of the rope was more due to internal cutting friction than from any external wear. The application of oil or other lubricant capable of permeating the rope was found to materially mitigate this internal wear. For example, one rope $1\frac{3}{4}$ in. circumference, ran over a

10½ in. diameter pulley 16,000 times unoiled, and with lubrication 38,700 times; while a similar rope unoiled ran over a 24 in. diameter pulley 74,000 times, and when lubricated 386,000 times.

Decrease in the sectional area of the wire in the rope, as well as the breaking of the wires, may be the result of some accident, such as:—

Derailment of the skip in incline shaft;

Mounting of the rope on the pit-head sheave;

Kinking of the rope during unreeling, or on occasions when the stress is taken off;

Impact with other bodies, falls of rock, etc.;

Burning by electric discharge from fallen cables, or other source.

In the case of any accident such as above mentioned, the rope would naturally be carefully examined, and no difficulty would be experienced in locating the damage sustained by it.

Rusting or corrosion also contributes largely to the wasting away of the substance in the rope.

It is not possible generally to keep winding ropes dry, and mine water very often contains free sulphuric acid and salts, such as ferrous sulphate and ferric sulphate, which accelerate the corrosion which would occur slowly, even in their absence, from the presence of water, oxygen and carbonic acid.

Analysis of water from one shaft† showed:—

(1) Free sulphuric acid = ·0257 per cent. or 18·0 grains per gallon.
Acid salts (calculated as H_2SO_4) = 18·4 grains per gallon.
Total acidity = 49·6 grains per gallon.

NOTE.—There is much *ferrous* sulphate present.

(2) Free sulphuric acid = ·0196 per cent. or 13·7 grains per gallon.
Acid salts (calculated as H_2SO_4) = 21·3 grains per gallon.
Total acidity = 48·8 grains per gallon.

NOTE.—Much *ferric* sulphate is present.

Doubtless many mine waters are worse than the above quoted. One analysis was referred to‡ last November in this Association that showed 120 grains of SO_3 per gallon.

The writers here put forward some chemistry notes dealing with the subject, and culled from the works of several authorities.

The corrosion of steel or iron is more essentially due to oxidation than to the direct solvent action of acids, although the latter enhances the severity of the attack. With iron immersed in sulphuric acid we have the action indicated by the equation:— $Fe + H_2SO_4 = FeSO_4 + H_2$, but the action is slow even in solutions containing up to 1 per cent.

On the other hand, the oxidation of steel or iron, commonly known as rusting, is especially rapid and far-reaching, because the rust first formed does not protect the rest of the metal, but rather increases its corrosion. The simultaneous presence of H_2O, O_2 and an acid (generally H_2CO_3) is necessary for the rusting of iron, but little rusting occurs in the absence of any one of these three. Moreover, the presence of an alkali, or even an alkaline carbonate, prevents rusting. The rusting takes place in two stages, as shown in the following equations:—

$$Fe + 2H_2CO_3 = Fe(HCO_3)_2 + H_2$$
(ferrous bicarbonate) (soluble in water)

$$2Fe(HCO_3)_2 + O + H_2O = 2Fe(OH)_3 + 4CO_2$$
(rust)

Since CO_2 is regenerated, a small quantity of CO_2 will rust an indefinitely large quantity of iron. With the H_2 liberated in the first stage, some of the carbon of the iron will be removed as hydrocarbons. Some of the hydrogen may be absorbed by the steel with deleterious results to the latter. Some of the iron in rust is present in the ferrous condition owing to the reducing action of the metal on the ferric oxide already formed. This ferrous hydrate in course of time oxidises to ferric oxide, and so the attack on the metal is progressive.

Equations:—

(1) $8Fe(OH)_3 + Fe = 3Fe_3(OH)_8$
(ferrosoferric oxide)

(2) $2Fe_3(OH)_8 + O + H_2O = 6Fe(OH)_3$

and so on alternatively, the ferrous oxide acting as a carrier of oxygen from the air to the iron, and greatly accelerating the action.

† See Appendix B, page 20, Government Mining Engineer's Annual Report, Part II, June 1904. Dr. Jas. Moir, D.Sc.

‡ Page 116, "Journal of the Mechanical Engineer's Association of the Witwatersrand." Mr. J. R. Williams.

With mine water containing ferric or ferrous sulphate we shall get quick wasting. For instance, ferric sulphate $Fe_2(SO_4)_3$ by alternate reduction and oxidation, will act as an oxygen-carrier and accelerate the rate of rusting of iron, the equations being:—

$$Fe_2(SO_4)_3 + Fe = 3FeSO_4 \quad \ldots \quad \ldots \quad (1)$$

$$2FeSO_4 + O + H_2O = 2Fe(OH)SO_4 \quad \ldots \quad (2)$$
(basic ferric sulphate)

$$3Fe(OH)SO_4 + Fe = 3FeSO_4 + Fe(OH)_3 \quad (3)$$
(rust)

The reactions (2) and (3) go on alternately. This explains to some extent the rapid action of what are known as acid mine waters, there being a great acceleration of the ordinary oxidation process.

In wrought or ingot iron the corrosion rises with the percentage of carbon, and this applies to steels with much combined carbon. Rusting is increased by galvanic action, as, for instance, tinplate when the tin coating is broken. Similarly with iron railings coated with lead, corrosion occurs just above the junction of the metals. The same applies to copper-covered iron, the protection being satisfactory only so long as the coating is perfect. Galvanised steel (with zinc), on the other hand, rusts less than steel, since zinc is attacked first, but it is very susceptible to chlorides (*e.g.*, sea water), and also will not stand the action of the free acid in some mine waters. The thin coating of zinc on the wires of a winding rope will very soon be worn through at the places where external or internal rubbing occurs.

In speaking of galvanised wire—*i.e.*, wire coated with metallic zinc to preserve it from oxidation—Mr. J. Bucknall Smith states in his Treatise on the Manufacture and Uses of Wire: "This process consists in drawing cleansed wire through a bath of molten zinc (spelter), and whereby it retains a metallic film, forming a galvanic pair or complement. By virtue of the positive properties of the zinc, this metal is attacked upon contact with air, moisture, or carbonic acid, and in this manner the iron or steel is preserved. Galvanised metal will not, however, stand exposure to acid vapours, such as produced by products of combustion, etc.

"Galvanised wire is largely used for most situations, excepting smoky and seaside districts, although it should be understood that the zinc coating is to some extent soluble in rain water; indeed, its protective influence appears due to the formation of a film of oxide. If, however, any imperfections exist in the galvanisation, the iron corrodes more rapidly than plain wire. . . . The elongational efficiency of iron and steel wire is slightly diminished by the process of galvanising."

A galvanic couple may arise from contact of two different kinds of iron, and even by lack of homogeneity in one piece of iron, two different varieties causing local galvanic action. Even variations in the proportion of Mn—that is to say, the manganese disposed irregularly—may be the cause of local action. Generally speaking, "pitting" effects are due to galvanic action, and the extent of deterioration is local.

An interesting discussion* is now quoted regarding the process of rusting and its prevention. "It has been proved that, though H_2O and O_2 are necessary for rusting, the presence of CO_2 is not necessary, though it may accelerate the action. The efficiency of alkali in preventing rust is not due to withdrawal of CO_2, but to the prevention of the formation of H_2O_2. Dry inert gases have no effect on iron, and even with moist gases no action occurs unless the temperature fluctuates, so that liquid water settles on the metal. Thus liquid water is necessary. Iron, immersed in water, saturated with H_2, N_2 or CO_2, did not rust, but in solution of O_2, or $O_2 + CO_2$, it rusted. The effect of chemicals in aiding or preventing rusting in presence of water and O_2 was investigated. The following prevent rusting:— Na_2CO_3, $(NH_4)_2CO_3$, borax, $Ca(OH)_2$, Na_2HPO_4, NH_3, $K_2Cr_2O_7$, K_4FeCy_6, CrO_3, K_2CO_3, and $NaNO_2$. Rusting occurs with the following:—NaCl, $KClO_3$, $FeSO_4$, K_3FeCy_6, KNO_3, and Na_2SO_4. The first series are compounds which destroy hydrogen peroxide, and the second series does not. H_2O_2 rusts iron very rapidly, also Zn and Pb, whereas Cu, Ag and Ni do not rust in air and are not attacked by H_2O_2. Several analyses of rust give the formula $HFeO_2$. In the "rusting" of zinc H_2O_2 can be detected, but not in case of iron, probably because it is used up by the iron at once.

(1) Direct decomposition of H_2O by iron requires a high temperature.

* "Rusting of Iron," W. R. Dunstan, "Proceedings of Chemical Society" (London), 1903, p. 150. Further details in "Proceedings of Royal Artillery Institution" (Woolwich), Vol. XXVI, 1899, No. 5; and in "Engineering," June, 1900; also in "Report (1900) of Steel Rails Committee of Board of Trade."

(2) Action of carbonic acid on iron liberates H_2 and forms $Fe(HCO_3)_2$. If O_2 is admitted the latter oxidises to rust and CO_2.

(3) Electrolytic action occurs if iron is impure, and H_2 is evolved. This action is not checked by alkali.

On the other hand, Dr. G. T. Moody stated: "The author finds that the substances which prevent rusting do not act by destroying H_2O_2, but by their influence on the absorption of CO_2, *e.g.*, H_2O absorbed 90·6 volumes CO_2, whereas 15 per cent. CrO_3 solution absorbed only 4·2 volumes, and 20 per cent. solution of $NaNO_2$ absorbed 5·6 volumes. As for CrO_3, its apparent protection is due to its property of dissolving ferric oxide—the metal is attacked but remains clean. O_2 and water do not attack iron until CO_2 is admitted.

The interaction of H_2CO_3 and iron is strictly comparable to that between H_2SO_4 and Fe. In a week a saturated solution of CO_2 with 500 grs. iron turnings yielded 635 c.c. of hydrogen, and the solution contained ·1 per cent. of Fe as bicarbonate.

The author contends "that the primary action in rusting is the oxidation of ferrous bi-carbonate previously formed from CO_2 and iron."

Dr. Jowett (collaborator of Mr. Dunstan) said that "rusting is the sum of several factors," and insisted that the formation of H_2O_2 is one of these factors. Although ordinary rust on analysis shows a little CO_2, it approximates very closely to $HFeO_2$.

Mr. Chapman suggested that part of Dr. Moody's hydrogen might be simply due to electrolysis.

In reply, Dr. Moody said that "wet iron in ordinary air must have a layer of ferrous bicarbonate on it, and as the latter destroys H_2O_2, Dunstan's theory can have no bearing on the practical question."

Dr. Moody, in final reply, stated: "Salts of strong acids—sulphates, chlorides, etc.—have no retarding effect on rusting, and such compounds have no affinity for CO_2. Substances which retard rusting are in two classes: (1) of alkaline reaction, sodium carbonate, hydrate, phosphate, borate, etc., which act by combining with CO_2 and removing it; (2) salts of weak acids, nitrates, formates, acetates, ferrocyanides, etc., which are sensitive to CO_2. The three metals rusting in air, Fe, Zn and Pb, are also ones which are attacked by carbonic acid. The author finds that *pure* H_2O_2 does not attack iron, but it is very rapid if CO_2 or a trace of acid is added."

In the compiling of the foregoing notes on corrosion the writers wish to acknowledge the very great assistance rendered to them by Dr. James Moir, Chemist to the Mines Department.

It will be seen that authorities are not entirely in agreement as to the part played by CO_2 in the corrosion process, but there is no possible doubt that corrosion of steel or iron does occur when CO_2 is present in suitable company, and also that the acids and salts contained in many mine waters lead to more or less rapid wasting.

It is hardly necessary to remark that CO_2 is always present in the atmosphere. It exists in the air to the extent of ·035 per cent. by volume. This percentage is found to be increased about three times in the air of the upcast shaft of a mine; it then becomes about ·1 per cent.

Sulphuretted hydrogen (H_2S) is not entirely unknown in mine waters. It is encountered occasionally in new workings when big rushes of water are struck. It probably collects in caverns, the drainage of which only takes place intermittently under ordinary circumstances—heavy rainfall starting the siphon flow. Its presence in mine waters will increase corrosive effects.

It is not left to the persons responsible for the examination of winding ropes to discover the presence of acid or other impurities in mine water, for in shafts where this enemy is present many other evidences force it to the notice of the management. In such cases special attention should be paid to the thorough dressing and examination of the rope. Whether any continuous treatment of the rope with alkaline solution would be effective in preventing the corrosion due to this class of mine water is a subject which may be left in the hands of the chemist.

The writers carried out some experiments with wires, immersed in a 10 per cent. solution (by weight) of sulphuric acid, to ascertain the effects (other than decrease in section) on the wire. Three strands were taken, one from each of three ropes of different manufacture. The wires of these strands were unlaid, straightened and cleaned. Each wire was then divided into two pieces of equal length. Piece 1 was carefully measured and tested for tensile

†Proceedings of Chemical Society, London (1903), p. 157.

strength, elongation, bending and torsion. Piece 2 was bent thus ⊓ to suit the accommodation in the acid bath in which it was then placed. After 91 hours one leg was nipped off, measured and tested. The two other legs were at once returned to the bath and left there for a further period of 50 hours. They were then removed, measured and tested. The dish was not stirred during the test. Particulars of test results are shown in Table V. The action of the acid solution was different in its effects on the wires. In some cases the corrosion occurred mostly as " pitting," and in the others as " long scorings " (see Figs. 1 and 2, Plate II). In all cases the action of the acid was much more violent in effect at the bends, the places where the wires were extra strained (see Figs. 3 and 4, Plate II). There was difficulty in measuring accurately the sectional areas of the wires after the corrosion, and for this reason not much reliance can be placed on the figures showing the breaking stress. Possibly the tensile strength was a little reduced, but the capacity of the wire to withstand torsions and bends was almost entirely destroyed.

Regarding (2), the deterioration due to " fatigue " or change of state of the steel, it is well known that the frequent application of a load well below that producing stress up to the elastic limit eventually produces " fatigue " in steel. Reversals of the load accelerate this action. The fatigue shows itself by the steel becoming brittle. Whether this is due to an alteration in the arrangement of the molecules of the steel, this changing from the fibrous to the crystalline structure is a point on which scientists do not agree. Most certainly the wire becomes brittle and behaves as if the structure of the steel had become crystalline.

Mr. H. Marion Howe, in the work already referred to in this paper, goes into the question very fully and quotes evidence on both sides. He opens the subject by asking (1) do vibration, etc., induce coarser crystallisation; or (2) do they, without altering the shape or size of the crystals, increase the tendency to yield a crystalline fracture?, and concludes thus:—

" To sum up, while vibration and shock often cause rupture under light stress, and while it is proverbially difficult to prove a negative, we have, I think, every reason to believe that the granulation and crystallisation of iron under vibration and shock is a myth. The word " iron " is used, but the sense " iron or steel " is evidently intended by the context.

The writers are not in a position to offer an opinion on this very debatable subject, and therefore pass on to consider some of the mechanical causes that lead to this " fatigue " in winding ropes.

The stress due to bending is probably the chief of these.

Mr. Biggart's experience went to show that the severity of bending largely determined the life of a rope, and that generally a rope lasted twice as long when only bent in one direction, as against those used under alternate stresses of flexure. It is well known in mining practice that the contra flexure exerted upon the rope that winds on at the lower side of the drum shortens its life in comparison with the top rope. It is also well known that, with proper sizes of sheaves, transmission ropes will last for several years.

The bending of the wires, as laid up in a rope, is less severe than if they were straight, the compound spiral form having the effect of lessening the bending stress. It is well to get a proper understanding of this action.

Suppose a pull P to act along a straight strand. If ω is the angle of lay of the wires, then the sectional area of wire cut through by a plane at right angles to the line of the strand will be sec ω times the aggregate sectional area of the wires.

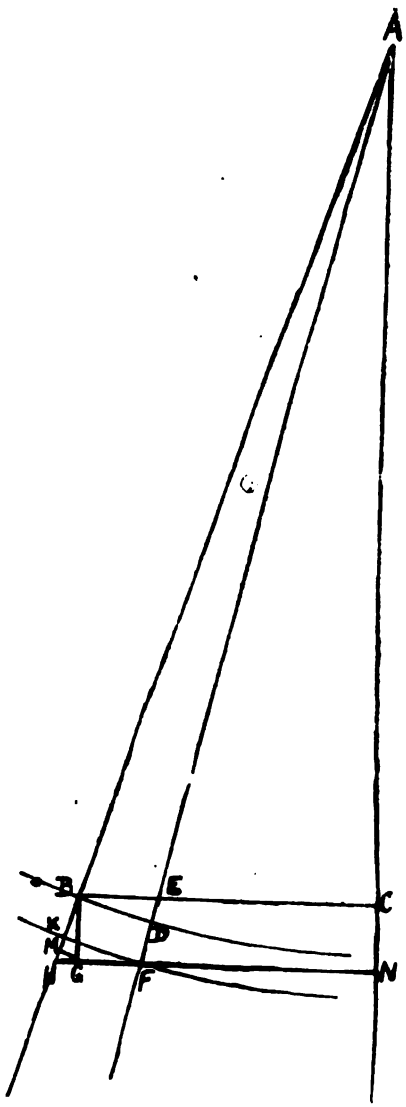
The pull P may be resolved into P cos ω acting along the wires and extending them, and P sin ω acting at right angles to, and tending to undo the strand—that is to say, to straighten out the wires. If any decrease of spirality occurs, as it undoubtedly will, P cos ω will increase, and P sin ω decrease, in proportion to the decrease in ω Some slight decrease in the circumference of the strand also occurs, and the accompanying figure may be taken to represent the effect in any one wire, this wire of the strand being supposed to be rolled off into a plane; the figure is exaggerated for the sake of clearness. The length of strand AC is increased up to N, while the length of wire is increased from AD (= AB) up to AF. The ratio of the elongation of the strand to that of the wire is as

$$\frac{CN}{AC} : \frac{DF}{DA}, \text{ or as } \frac{CN}{AC} : \frac{BK}{AB}, \text{ or } \frac{CN}{AC} : \frac{BK}{AC} \sec \omega$$

i.e., as CN : BK cos ω.

Now, because the angle HBG is equal to the angle BAC = ω, therefore BM = BG cos ω = CN cos ω.

For a very small decrease of the angle w, that is to say, if the line AF lies nearer to



AH, then BK will very nearly equal BM, which we have seen is equal to CN cos ω. Thus we may say that practically BK = CN cos ω. In this case the ratio CN : BK cos ω becomes CN : CN cos² ω

or 1 : cos² ω,

which is the result arrived at mathematically by Hrabak, who, however, does not allow for decrease in circumference of strand, but keeps the base FN equal to the base BC.

Hrabak uses a_1 to represent the fractional elongation of the wire, and a that of the strand. He obtains the result $a_1 = a \cos^2 \omega$ for the wires in a strand, and $a_1 = a \cos^2 \omega \cos^2 \omega_2$ for the wires in a rope. If the angles (ω and ω_2) of the "lays" in the strand and in the rope are about equal, then $a_1 = a \cos^2 \omega \times \cos^2 \omega = a \cos^4 \omega$. For a cable, a three times twisted rope, we have the value $a_1 = a \cos^6 \omega$.

Using the letters E_0, E and E_1 to represent the modulus of elasticity of the straight wire, the modulus of the rope, and of the wire as laid up in the rope, Hrabak obtains from the results of experiments and calculations, all of which are fully corroborated by the independent researches of others:—

	For Strands.	For Ropes.	For Cables.
E	$= 0{\cdot}6\ E_0$	$0{\cdot}36\ E_0$	$0{\cdot}216\ E_0$,

and from his own calculations,

if $\omega = 18°$			
a_1	$= 0{\cdot}904a$	$0{\cdot}818a$	$0{\cdot}74a$
$\frac{1}{a_1}$	$= 1{\cdot}106\frac{1}{a}$	$1{\cdot}222\frac{1}{a}$	$1{\cdot}351\frac{1}{a}$
E_1	$= 1{\cdot}106\ E$	$1{\cdot}222\ E$	$1{\cdot}351\ E$,

whence it follows:—

E_1	$= 0{\cdot}6636\ E_0$	$0{\cdot}44\ E_0$	$0{\cdot}2918 E_0$

E_0, the modulus of elasticity of straight wire (if of plough steel) is equal to about 28,500,000 lbs. per sq. in. The value of E_1 follows from this, for the wire in the rope, as $\cdot 44 \times 28{,}500{,}000 = 12{,}540{,}000$ lbs. per sq. in.

The bending stress may now be calculated for a rope containing wires of δ in. diameter, going over a sheave of D in. diameter, the formula being:—Stress $= E_1 \frac{\delta}{D}$.

If the ratio $\frac{\delta}{D}$ is equal to $\frac{1}{1200}$ the bending stress will equal 10,450 lbs. per sq. in.

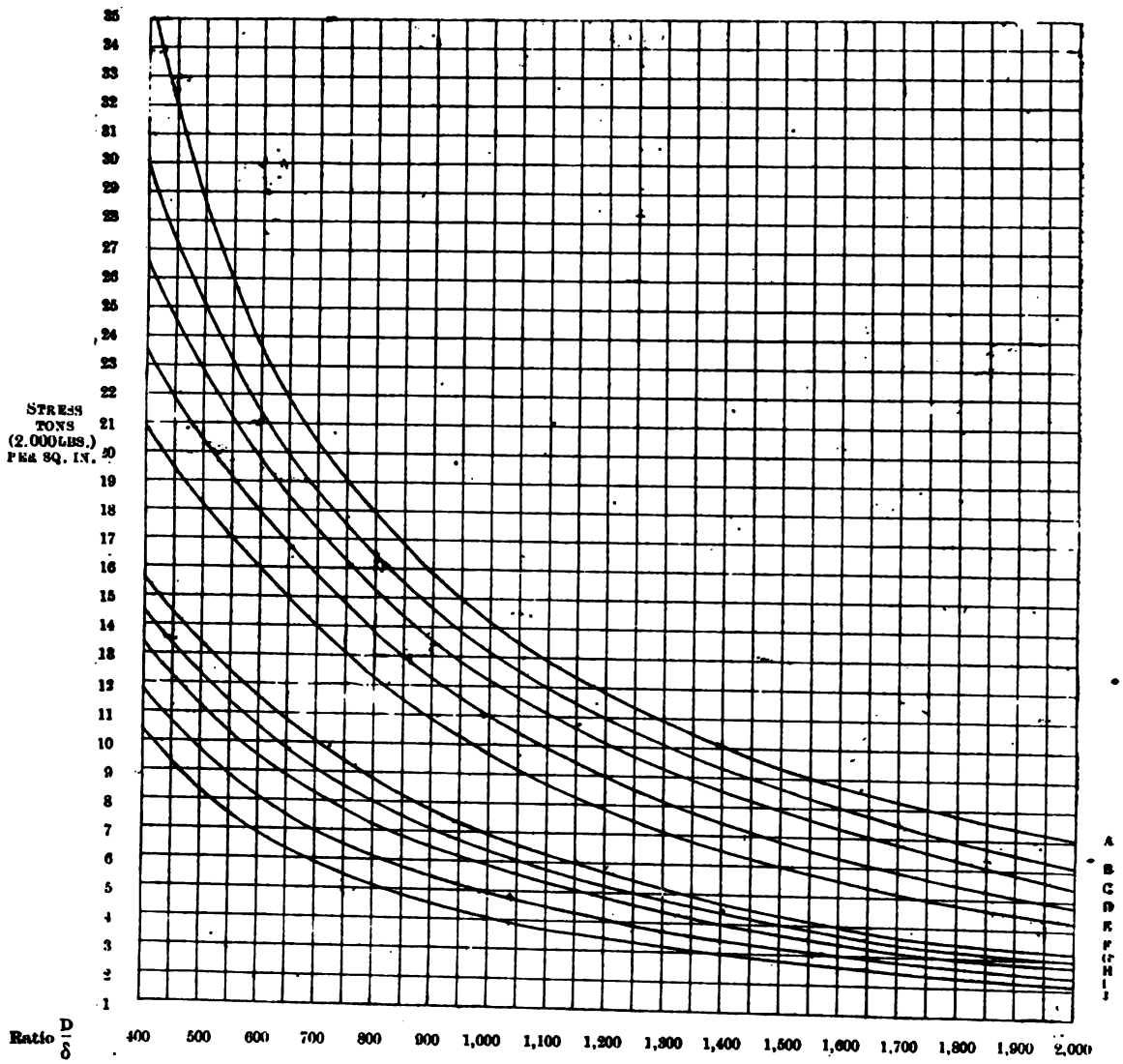
Diagram 1 shows the bending stress per sq. in. in the wires of *new* ropes of various constructions, according to Hrabak's assessment.

It may be seen from the diagram how unfavourably Hrabak regards ropes that have wire cores in their strands, and also the high bending stresses that he assesses for the wire in flat ropes.

It will be noticed in the foregoing remarks that no consideration has been paid to the size of the rope in which the wires occur. It appears to the writers that, the size of a wire remaining the same, its occurrence in a rope of large diameter

DIAGRAM I.

Bending Stress in Wires of New Ropes.



Wire. (Straight).	Lock Coil Ropes	Wire Spiral Ropes.			Round Ropes with Hemp Cores in Strands, including Strand Spiral Ropes.				Cables
	Flat Ropes with Wire Cores in Strands.	Flat Ropes with Hemp Cores in Strands. Round Ropes with Wire Cores in Strands. Normal	Flexible.	Very flexible.	Normal	Flexible.		Very flexible.	
	A	B	C	D	E	F	G	H	I	J
Tons per sq. in.	$E = 14,220$	$E = 11,600$	$E = 9,950$	$E = 8,540$	$E = 7,300$	$E = 5,120$	$E = 4,660$	$E = 4,200$	$E = 3,640$	$E = 3,070$
	$E_1 = 14,220$	$E_1 = 12,100$	$E_1 = 10,650$	$E_1 = 9,450$	$E_1 = 8,400$	$E_1 = 6,150$	$E_1 = 5,800$	$E_1 = 5,350$	$E_1 = 4,740$	$E_1 = 4,150$

would be more favourable than in a rope of smaller diameter.

This remark is merely advanced to question in a small way the completeness of Hrabak's theory. As a matter of fact, considering ropes of the same construction, in the larger ropes larger wires always occur, but in the cases where alteration in construction allows of the same size wires being used in large as in small ropes, the large ropes should give more favourable conditions in the matter of bending.

Another point worth mentioning is that Hrabak's theory only applies to the wires as they appear on the outer surface of the rope. In proof of this statement the following facts may be advanced.

If we assume, what is generally very near the truth, that the angle of lay of the wire in the strand is the same as that of the strand in the rope, we find that, when laid up in the form of rope, the angle made by the wire with the axis of the rope varies from 0 to 2 ω.

In a Lang lay rope the angularity (2 ω) occurs in the wire when it is in the crown of the strand, and the value 0 occurs when it is on the opposite side—*i.e.*, touching the hemp core. The angularity of the wire varies around the circumference of the strand from 2 ω to 0, in each direction.

With rope of ordinary lay the angularity of the wire with respect to the axis of the rope is 0 on the crown of the strand and 2 ω on the opposite side.

If the truth of this fact is difficult to grasp, an inspection of the cores of ropes of both lays will speedily prove convincing—the bedding marks of the wires will be plainly seen, practically parallel with the axis in the Lang's lay rope, and at an angle of about 2 ω in the rope of ordinary lay (see accompanying illustration).

Bearing in mind that the bending stress in a wire is greater the nearer this wire runs parallel to the axis of the rope, it will be evident why, in the case of a rope of ordinary lay, the wires break so readily on the crowns of the strands. In this position they are not only parallel to the axis, but also farthest away from it. This combination of unfavourable conditions does not occur in a Lang lay rope, and, moreover, where the wires do lie parallel to the axis of the rope, and are therefore badly situated as regards bending, they have the relief afforded by their bedding on the hemp core of the rope.

It may be stated that owing to varying angularity of the wires composing the rope, the actual cross section—cut by a plane at right angles to the axis of the rope—is sec.$^2\omega$ (*and not sec. 2* ω) times the aggregate sectional area of all the wires.

The writers now pass on to consider some of the " lays " and " angles " met with in practice.

From Mr. Bucknall Smith's treatise, before referred to, the following extract is quoted:—

" The following information respecting the manufacture of successful haulage ropes of useful sizes, made according to the construction last described (Lang lay), will be found instructive:—

PHOTOGRAPHS OF HEMP CORES.

ORDINARY LAY. LANG'S LAY.

Approximate Diameter.	Circumference of Rope.	Number of Strands.	Number of Wires in Strands.	Number of Wires in Core.	Gauge of Wires.	Lay of Strands.	Lay of Ropes.
in.	in.				in.	in.	in.
$0\frac{5}{8}$	2	6	6	1	·072	$2\frac{1}{4}$	6
1	3	6	6	1	·105	3	$7\frac{1}{2}$
$1\frac{1}{4}$	4	6	8	7	·116	$3\frac{3}{4}$	9
2	6·2	6	10	7	·146	$4\frac{3}{4}$	13

"Upon analysing this practice it will be noticed that the proportions the lays in the strands and ropes bear to the diameter of the roping range from about three and a half to two and a half and six and a half to nine times the diameters respectively—*i.e.*, in order to maintain a requisite degree of flexibility the lays are reduced as the sizes of the cables are increased."

The angle of lay (ω) in the above quoted cases increases from about 18 deg. for the $\frac{5}{8}$-in. rope to 24 deg. in the case of the 2-in. rope. The size of the wires in these cases is not mentioned.

The "lays" noticed by the writers in new ropes of various constructions correspond generally with those mentioned in the table above. The length of lay increases, and the angle of lay decreases, in the case of a worn rope.

Turning now to consider partially-worn ropes, it is found that after a certain time in use the spinning effect in a rope is to a great extent eliminated. This corresponds to effects noticed during tests of samples of worn rope under tensile load. There is very little, if any, untwisting apparent. This may be due to the falling off in the elasticity (*i.e.*, a raising of the modulus) in the material of which the wire is composed, to permanent set in cases where wires are very much worn, and therefore have small sectional area remaining, or to the flattening of the strands and wires, or to the general increase, from one cause or another, of resistance to untwisting. Strands with hemp cores might be expected to flatten out and lock more speedily than those with wire centres.

If in a worn rope no untwisting effect takes place, we shall have $E_1 = E$. But on the other hand E will be more nearly equal to E_0; always remembering that the wires present a cross sectional area at right angles to the direction of loading of $\sec^3 \omega$ times these aggregate areas. $\sec^3 18° = 1{\cdot}1$, so to E, and therefore E_1, may approach very nearly equal to E_0. This means that the bending stress in the wires of an old rope may be much more severe than in those of a new rope. This point Hrabak also considers, but the writers have not been able to secure a full translation of his very useful treatise. However, diagram 2 shows his results.

Table VI., taken from the Government Mining Engineer's Annual Report, 1903, gives the general values existing on these fields for $\frac{D}{\delta}$.

Table VII, taken from the same source, shows the alteration that the inclusion of bending stresses makes in the factor of safety. These are assessed on the ropes considered as slightly worn, and the values will be decreased as the ropes wear further.

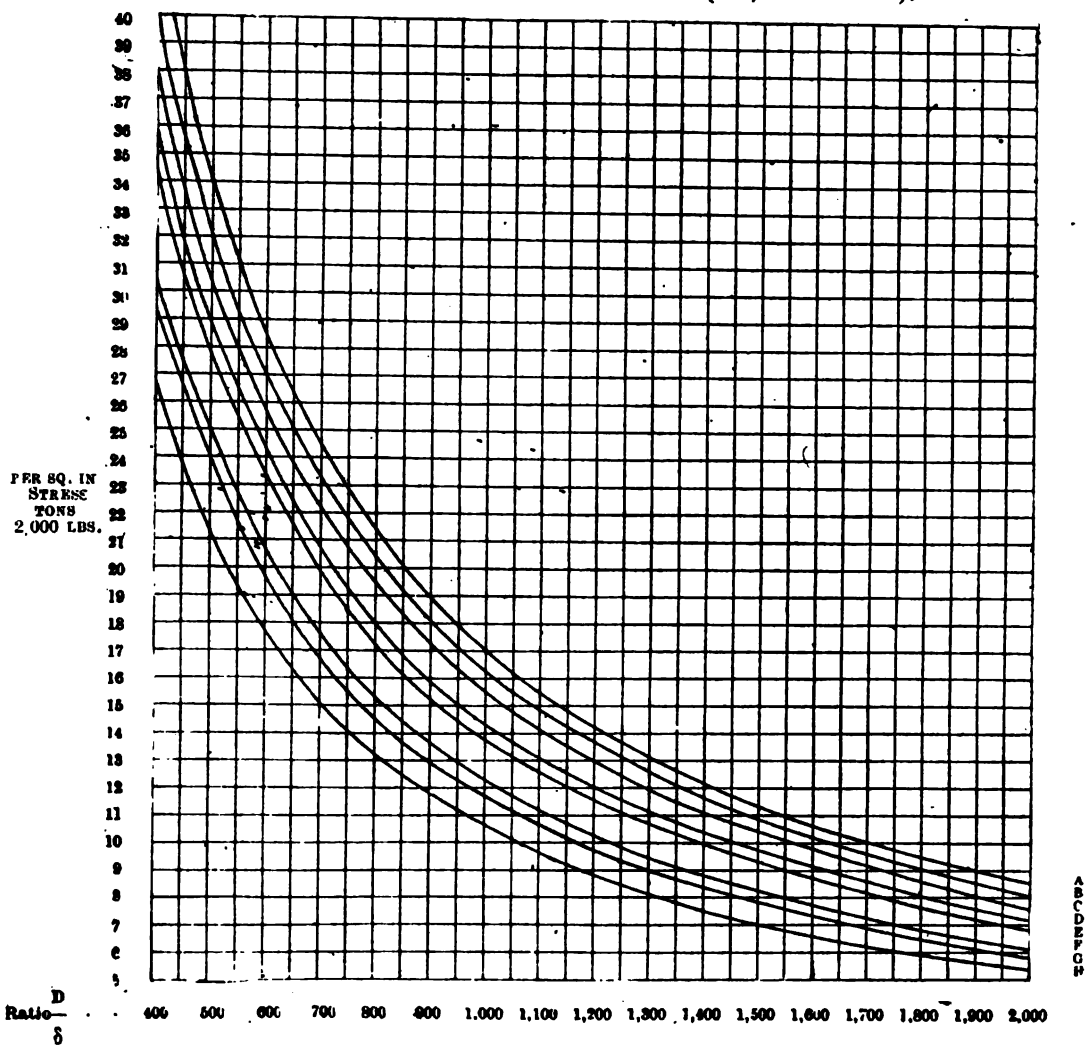
In local practice it has certainly been found that the brittleness in a rope is much more marked when it is subjected in winding to a large number of reverse bends, and it is very probable that a winding engine with a light drum will also cause the same results on account of the uneven turning moment on the drum shaft. The writers have examined one winding plant where these results have occurred under the conditions stated. In this case the ropes only last about 16 or 18 weeks; broken wires begin to appear after the rope has been about a month in use, after which time the number increases very rapidly from week to week, until, in about the 16th or 17th week, as many as 10 wires break each day.

It is found that the condition of the wire is such that one 180 deg. bend will break it, whereas it took eight or nine bends to accomplish this when the wire was new. In this particular case there was very slight frictional wear on the wires, which were practically their full size.

In another case micro-photographs were taken of wires in a rope, samples being cut from a very much worn part, and also from the end which had been coiled spare on the drum, and which had therefore been subject to very little stress, and it was found that in the former case the molecular structure was entirely different from that in the latter. In this instance, however, the wires

DIAGRAM 2.

BENDING STRESS IN WIRES OF OLD ROPES (*i.e.*, AFTER USE).



	Wire. (Straight).	Lock Coil Ropes.	Flat Ropes and Wire Spiral Ropes.			Round Ropes with Hemp Cores in their Strands, and Strand Spiral Ropes.		Cables.
			Round Ropes with Wire Cores in Strands.					
			Normal.	Flexible.	Very flexible.	Normal.	Flexible.	
	A	B	C	D	E	F	G	H
Tons per sq. in.	E = 17,000	E = 15,000	E = 14,000	E = 13,000	E = 11,800	E = 10,000	E = 9,200	E = 7,900
	E_1 = 17,000	E_1 = 16,000	E_1 = 15,000	E_1 = 14,000	E_1 = 13,500	E_1 = 12,100	E_1 = 11,600	E_1 = 10,700

were much reduced in size, due to wear, frictional or corrosive, or both.

The former case cited, in which the rope has been put out of use on account of a very large number of breaks in the wires, is not a solitary case. Until further information as to the real cause of the deterioration is furnished, it is popularly attributed to "fatigue" of the steel, and it is generally supposed that the more bendings there are in the journey of the rope, and the more frequent reversals occur, the shorter the life that may be looked forward to for the rope.

There are, however, other causes that may tend to the peculiar effect of loss of ductility, while original size and appearance remain practically intact.

Henry Marion Howe, in his exhaustive treatise on the Metallurgy of Steel, states:

"Exposure to nascent hydrogen greatly diminishes the flexibility of wrought iron and steel, and to a much smaller degree that of cast iron, and the transverse strength of steel and probably of the other varieties of iron. The elongation of the metal is often simultaneously diminished, though usually to a very much smaller degree, and it is in general affected in a way which is much less clearly understood; while the tensile strength and modulus of elasticity are affected but slightly, if at all.

"The fracture of metal which has been thus exposed, if moistened while still warm from the effort of breaking, froths and gives off copious bubbles for 30 or 40 seconds, and even the unbroken metal evolves gas bubbles when first immersed in water, especially if the latter be warm. The frothing power is destroyed, and the flexibility nearly, and perhaps quite completely, restored very rapidly by heating the metal, slowly by simple exposure to the atmosphere at ordinary temperatures.

"Some or all of these effects are produced when iron is immersed, A, in hydrochloric, sulphuric or acetic acid, in the two former even if extremely dilute; B, in mine water; C, if employed as the hydrogen pole (cathode) in electrolyzing common water, caustic soda, hydrochloric, sulphuric, or indeed any acid, or neutral salts, the iron becoming very brittle though wholly uncorroded; while if employed as the oxygen pole (anode) it is greatly corroded but does not become brittle. Moreover, the metal exhibits the characteristic frothing after employment as cathode, but not after acting as anode. D, if exposed to the weather. E, in electrolytically deposited iron, some, at least, of these effects are greatly exaggerated."

In support of these facts H. M. Howe gives the following authorities: W. H. Johnson, A. Ledebur, D. E. Hughes and Stroh.

These effects are not due to corrosion, but to the exposure to nascent hydrogen. This is shown by several facts:—

(1) In the case of iron electrodes the effects vary directly as the exposure to hydrogen, but inversely as the corrosion.

(2) The effects are hastened and intensified by means which increase the evolution of nascent hydrogen; among these we have the passage of an electric current, and the contact of the iron with metallic zinc. The latter intensifies the brittleness, both in the case of immersion in acidulated water and of exposure to the weather, though it simultaneously diminishes the corrosion of the metal.

(3) That heating and rest, which expel to some extent the hydrogen, also remove these effects.

(4) That on filing away the exterior of the metal the interior is still found brittle.

(5) That immersion in nitric acid, which does not ordinarily lead by its action on iron to the liberation of hydrogen, does not render the metal brittle, though it rapidly corrodes it.

The nascent state is essential to these phenomena, as may be proved by experiment.

It may be mentioned that the purest iron attainable is that prepared by electrolysis, and by the subsequent heating in vacuo to expel the hydrogen with which it is always associated in this process of deposition. The amount of hydrogen thus combined amounts to about 240 or 250 times the volume of the iron, corresponding with formula $Fe_{13}H_2$. This form of iron containing hydrogen is hard and brittle, and is used for facing copper plates used for electrotype printing processes. Half of the hydrogen in electro deposited iron will escape when the metal is exposed to air at the ordinary temperature.

Mr. L. C. Moore* states, in dealing with the "pickling" process of the rods and

* "The Use and Abuse of Wire Rope." Proceedings of the Engineer's Society of Western Pennsylvania, 3rd January, 1905.

large wires during the drawing processes:— "The rods must now be baked, as, during the cleaning process, they have taken up a certain percentage of an element from the sulphuric acid, and they cannot be drawn until all this has been eliminated, and this can only be done by baking, and requires a heat of about 400 deg. F., and for a period of not less than 10 hours." This seems to point to an absorption of hydrogen, and a consequent brittleness which has to be removed, and apparently can be removed, by annealing.

Regarding the third cause of the deterioration of a winding rope, viz., discontinuity of the wires, sufficient has already been said on the subject of what are the probable causes of these breaks. It may be added, however, that unequal temper in the wire (the result of faulty manufacture) and unequal tension applied at the time of the laying up of the strands or the rope are defects tending to produce broken wires very early in the working life of the rope.

It is a dfficult matter to say how far a rope has lost strength due to there being a number of breaks in its wires. In any given length it is even difficult to discover how many individual wires are affected. If all the wires are of the same grade material it would be reasonable to assume that, where several had broken, the remainder must be strained near their breaking point. There is no doubt that a certain number of breaks in a given length of Lang's lay rope means more in the way of reduction of strength than in the same length of rope of ordinary lay, for the reason that the pinching of any single wire, due to contact with those of adjacent strands, occurs in any given length at shorter intervals of length in the latter case. Mr. Bucknall Smith quotes a case in which a rope sample, with all its component wires cut some 18 in. in advance of one another, lost very little in strength, as proved under test. This was possibly a rope of ordinary lay, and a new one. The writers will endeavour before the discussion on this paper closes to make some tests on samples of new rope with certain of their wires cut.

They present now Table VIII, showing the results of complete tests of 24 samples of worn ropes. These tests were made in the following manner: In the samples supplied (with ends well "served") about 8 ft. long, division was made so that three pieces of about equal length were secured. One piece was put aside for reference, the second piece was tested as a sample of the whole rope, and the third piece was divided up into strands. Of the six strands, four were tested whole, and the wires of the two other strands, after straightening and gauging, were tested singly, care being taken that each wire was broken in its most worn place. The results of all the tests of the wires of each rope were averaged and multiplied by the number of wires in the rope in question. This aggregate strength of the wires, *at their most worn places*, was compared with the load at which the whole rope broke, and also with the breaking load of the rope as assessed from the test of four strands.

It will be evident to all that if *lateral support* or *side* friction of the wires in a short length is of any value as constituting strength in a worn rope, then there should be some big difference between the actual breaking load of the whole rope and the aggregate strength of the wires (at their most worn sections).

It will be seen, however, from the results set out in Table VIII that out of 24 tests, in 16 instances the *whole* rope gave the lower breaking load, while, in the other eight cases, with one exception, there was very slight difference.

From the method, described above, of conducting the test, it will be seen that there possibly exists some slight difference in wear between the two samples from the same piece of rope, one of which is tested for strengths of wires and strands, and the other tested whole. One can only go by averages in this matter. The external appearance of some of these ropes may be judged from the photos on Plates I, II and IV, the reference numbers under the photos corresponding with those quoted in the first column of Table VIII.

It appears, therefore, that no serious consideration can be given to the effect of lateral support in the wires of a worn rope, but that the strength of the rope should be assessed from the amount of steel remaining at the most worn places in each wire.

It is very noticeable in the fracture of a worn rope under tensile test that the rope does not break clean across any one plane section, neither do the strands break in this way, but the parting takes place along the lines of least resistance, the lines of fracture running down the wires and finding out the weakest spot in each; generally this line is along the rubbing crowns of the strands. In

the case of a new rope the fracture occurs either clean across the rope or clean across two or three strands.

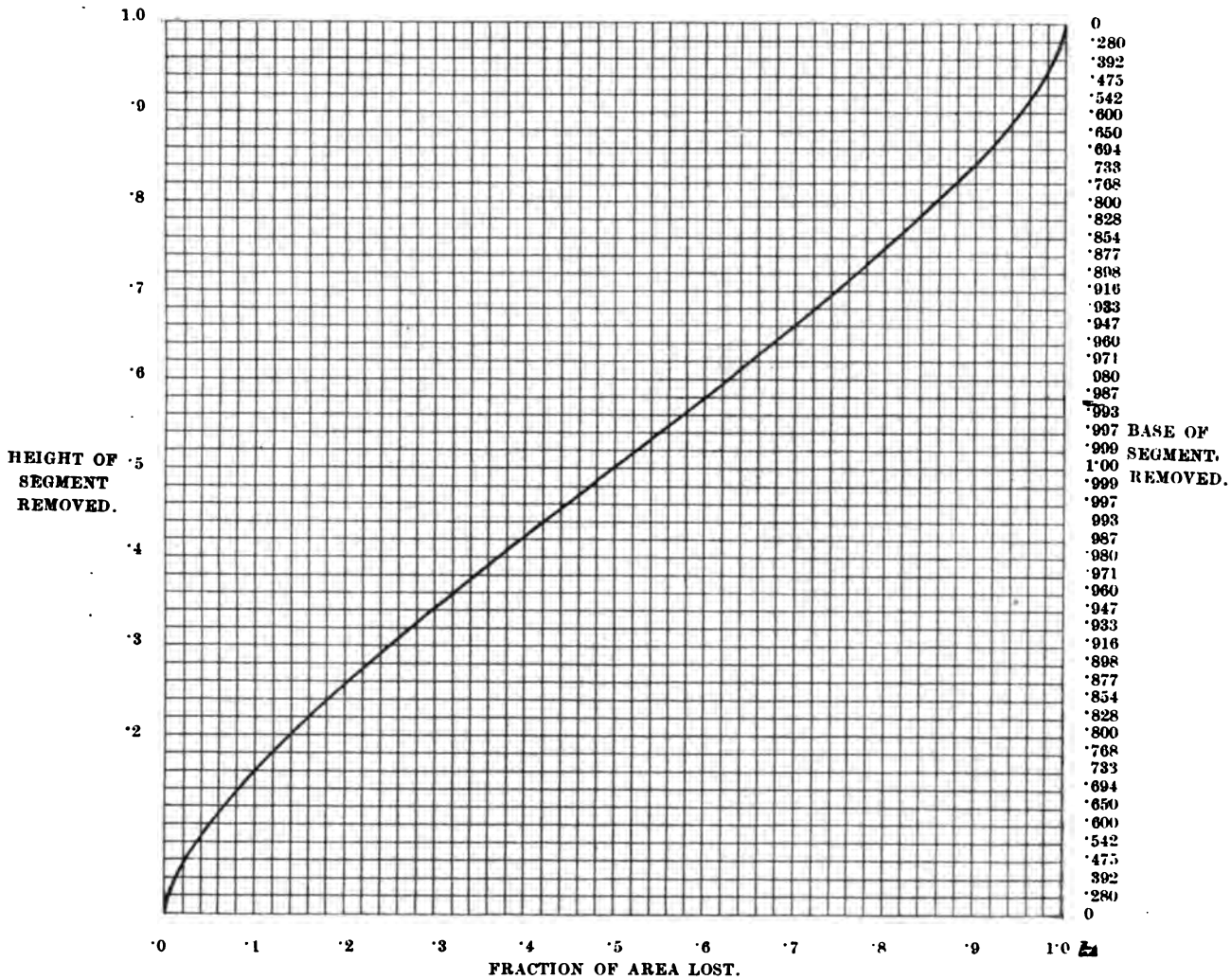
In Plate I the above-mentioned fractures may be seen in the photographs of ropes broken under test. Fig. 2 shows the characteristic fracture of a worn rope; and Fig. 3 of a new rope.

must be thoroughly cleaned at selected places.

It is the practice on several mines to make the thorough examination every week, and in this way the rate of deterioration of the rope is very carefully watched.

For the thorough examination of the winding rope it is necessary that the wind-

DIAGRAM 3.



To ensure safety in the use of winding ropes, their frequent careful inspection is absolutely necessary. The Mining Regulations of the Transvaal prescribe, in addition to a daily examination, that at least once a month the structure of the rope shall be examined for the purpose of discovering the amount of deterioration of same, and for the purpose of this examination the rope

ing engine should be worked slowly (*i.e.*, at a speed certainly not more than 1 ft. per second), with the necessary signalling arrangements provided, so that the motion of the rope may be stopped immediately in order that a minute examination can be made at any desired spot.

It is obviously necessary that when a rope is to be thoroughly examined all grease and dirt on the exterior must be removed.

It is desirable that the inspection of the rope should, as far as possible, be always carried out by the same person, and that explicit notes should be made in writing of its condition.

Regarding external wear, places in the rope that should be especially watched are those where contact with sheaves and pulleys occurs at acceleration or retardation periods of the hoist, and the writers consider that generally these places would also be likely to exhibit the greatest internal wear, excepting perhaps that portion of the rope nearest the skip or cage, which, although subjected to no rubbing wear, yet is exposed to the effects of internal drainage of moisture in the rope. If the conveyance is landed on chairs the release of stress at this end of the rope actually leads to a suction effect in attracting moisture.

At a position in the rope where it is desired to examine internally, this part should be brought to a point a little above the collar of the shaft and the weight of the conveyance and dependant rope should be taken up by means of wooden-faced clamps resting on beams across the shaft. It is then possible, by means of two "Spanish windlasses," to turn the rope in opposite directions, this leading to untwisting of the rope in a length of, say, three or four feet, and allowing the internal examination to be carried out. The lever bars used for the windless should be secured to the rope by hemp lashings, which also provide a protected fulcrum, and the wires of the rope will not be injured.

Plate III shows a photograph of a portion of rope under internal examination. The loosening of the strands is entirely "taken up" when the statical load is returned to the rope.

In estimating the reduction of the size of any wire due to wear, Diagram 3 may be of some assistance. This diagram shows the fractional area of circle remaining after a segment of known base and known height has been removed. Taking the diameter of the circle as unity, there are for each value of the base two values for the height—one greater than 0·5 and one less than 0·5. There should, however, be no uncertainty or error due to this cause, as it is a fairly easy matter to see whether a wire is half used through or not. The length of the base of the segment worn away can be measured by the person examining the rope after the removal of any side "burrs." To estimate the height of the segment worn away in a portion of rope which is fairly uniformly worn, the writers advise a comparison of the circumference of the rope at this worn part with the circumference of the rope at some place at either end of the rope where there is little or no surface wear, while at the same time there is the reduction in size of the rope due to untwisting and stretching under the working load. To measure these circumferences, wrap a fine wire or unstretching cord ten times round the rope in each case. Dividing each measurement by ten, the true circumferences are found, from which, by reference to Tables, the diameters of the rope at the two places under comparison may be arrived at. Subtraction of these diameters should give approximately twice the height of the segment of wire removed by wear. If the approximation is nearly correct, and if the base of the segment has been correctly measured, *these two values* (height and base) must lie at the ends of the same horizontal line in Diagram 3. If they do not, there is some error, and one must try again. If they do, the point where this horizontal line cuts the curve indicates the fraction of circle remaining, and also the fraction that has been worn away.

In a rope of simple construction every wire appears at the outer surface within a short distance, and the wear on each can be assessed in the manner just described. It is only necessary, then, to multiply what is considered to be the average amount worn away by the number of wires, and the total wear is estimated. From this follows directly the loss of strength, and a comparison with the original strength of the rope gives the strength remaining in the rope. For instance, if the wires have 25 per cent. of their area worn away, then the rope has lost 25 per cent. of its original strength.

It will be noticed that the writers attach no imporance to the possible effect of lateral support between the wires. The reasons for this step have been given earlier in the paper.

If the rope is of some compound construction—that is to say, if it contains other wires besides the core wires that do not ever come to the outside of the rope, and therefore are not subject to what has been called "external" friction—then it is necessary, before assuming that these inside wires are sound and intact, to

see that they are so by opening out the rope in the manner before mentioned.

When some of the outside wires are discovered slack—*i.e.*, " standing up "—it is generally found that the inside wires have deteriorated, though whether as the result or the cause of the first observed effect it is not quite clear—probably the former—and almost certainly so in the case of deterioration due to corrosion.

The estimation of the remaining strength in a worn rope is a very difficult matter in the case of a compound rope in which there is internal deterioration; but if there is none of the latter, then of course it is easy. Suppose the rope to be composed of six strands of 8/6/1 construction. If the outside wires are worn 25 per cent. and the inside wires are good, then the remaining strength in the rope is equal to the original strength of the inside wires *plus* 75 per cent. of the original strength of the outside wires.

In case of local corrosive action, such as pitting, the effect of this must be judged as the sum of the effects observed in the single wires, in a length of, say, three feet, and it will be wise and prudent to imagine no lateral support.

In case of general corrosion, examinations and tests of the portion of the rope removed at " re-capping " should afford a fair guide as to the strength of the rope.

The writers cannot too strongly recommend the practice of " re-capping " at three-monthly intervals. One advantage of the practice has just been stated; another is that the rope is, so to speak, " shifted along," and the positions of greatest wear are altered; while a third important gain lies in the removal of that portion of the rope that has been subject to the most severe kinetic stresses.

Regarding the loss of strength in a rope due to broken wires the writers have already said something. Until further tests have been made they do not feel in a position to say more.

Quoting from an article written by Mr. Charles W. Comstock* it is stated:—

" If tar is used as a protective coating, close attention is necessary to detect broken wires. The practice in France is to deduct the greatest number of broken wires in any one meter of length from the total number of wires composing the rope, and compute the factor of safety of the remaining wires. If this factor is found to be less than eight, the rope may be continued in service, but is no longer used for handling men. If it is as low as five the rope is retired from service. In this country (U.S.A.) smaller factors are used for new ropes, and there is no uniformity in the methods for determining when a rope should be replaced, the common practice being to use a rope until it breaks."

Without further knowledge the writers would recommend that the total number of wires broken in a length of three feet should be taken to mean a corresponding reduction in the strength of the rope. If it cannot be ascertained with certainty that the fractures occur in separate and distinct wires, they should be assumed to do so. To make their meaning perfectly clear: If in a rope of 36 wires (neglecting six soft core wires) there are found six breaks in the wires of a length of three feet, and it cannot with certainty be determined that the same individual wire is broken in more than one place, then the rope should be considered to have lost strength in this part to the extent of 6 divided by 36, *i.e.*, 16·6 per cent.

THE PRESIDENT (Mr. A. C. WHITTOME): It is hardly necessary to call attention to the important subject which has been so ably dealt with by the authors of this paper. The subject of wire ropes has been before us on two other occasions within the past few months, and that fact alone would show the interest our members take in it. This paper is far too important to be discussed hurriedly, and in fact the mechanical portions of it cannot be adequately handled until the tables and diagrams are before our members, but I may say we hope to have a thorough discussion on the chemical side of the paper this evening. In speaking generally of the paper, I should like our engineers to thoroughly consider the very important points which have been emphasised by the authors. Taking these in sequence, the first is a comparison of the service performed by ropes in different shafts or on different winding engines. We must agree with the authors that it is only when the duty performed by ropes is identical that it is permissible to compare their performances on the basis of total ore hoisted, but I do not think we would obtain a correct

* Mines and Minerals. April, 1904. Page 448.

estimate by the use of the formula which they give—mileage run divided by the factor of safety. I do not think any estimate of the duty of ropes can be considered as suitable unless the cost of the ropes figures as a component of the factor. Neither can it be deemed wise to consider only the tonnage hoisted during the life of the rope. The mileage run does not depend on the tonnage hoisted, and I think that component factor should be changed from miles into ton miles. Then again it would not be fair to compare ropes working in shallow shafts with those in deep shafts without taking into consideration the number of trips, because of the stresses due to the acceleration and retardation of the load. I venture to hope that a satisfactory factor for determining the value of ropes under different conditions of work may, during the discussion, be arrived at. The second point which struck me is the factor of safety. When I say factor of safety, I mean the Government Regulations regarding the factor of safety, and I was very pleased to hear the authors' remarks on this important point. It appears to me that we are working under very difficult conditions in taking as the authors say an arbitrarily high or low factor of safety. To argue from an analogy, on a railway system in foggy weather signals can only be conveyed to the engine-driver by detonating signals, and as a natural result trains run slow and the service becomes disorganised. It seems to me, as regards this factor of safety question, that we are still in the foggy phase and meet with delays and accidents, and we may hope that Messrs. Vaughan and Epton will help us to arrive at conditions approximating more to daylight. The authors were also emphatic on the desirability of the standardisation of rope specifications. As mentioned at our last meeting, this matter has been taken up by the Standardisation Committee of the Chamber of Mines, and we rely particularly on Mr. Vaughan, who is a member of that Committee, to have that matter satisfactorily arranged. Another important point was dealt with when the authors cited the various circumstances which cause external wear on the ropes. I had the satisfaction or dissatisfaction, some little time ago of seeing some very peculiar sheaves installed in a shaft at the turn from the vertical to the incline. These sheaves were so heavy that after eight or nine months a groove had been cut about 1¼in. deep by 1¼in. diameter ropes, and one gentleman expressed the opinion that they were heavy enough to cause an overwind. One section of the paper which is very important is that dealing with chemical action as causing deterioration of ropes. We are honoured by the presence this evening of some of the most eminent chemists on these Fields, and I propose to call upon them to contribute to the first section of the discussion. The final point which struck me in this paper is the emphasis which is placed by the writers on the necessity of a close inspection of the rope at frequent intervals. I would like to mention, having the sanction of Mr. Vaughan and the engineer of the mine interested, a very peculiar circumstance with regard to a piece of rope handed over to the Government laboratory for testing. Before it left the mine various opinions were given by men, all of whom were competent to judge, as to the breaking strain of that rope. These estimates varied from 18 to 28 tons, the original breaking strain being from 62 to 65 tons. When it was broken the testing machine recorded, I believe, about 40 tons. If the estimates of men who are quite competent to judge are from 30 to 60 per cent. out, I do not see how anyone can contend that an average engineer should be able to estimate within 10 per cent. I should like one or two of our American friends to tell us if the extract from Mr. Comstock's statement, as far as it deals with discarding ropes, is correct as regards American practice. I think we have too high a respect for American engineers to think that this is quite correct, and they will have an opportunity to say whether it is so or not. In declaring the paper open for discussion, I tender to the authors the heartiest thanks of the Institute and congratulations for the excellent paper they have presented to us.

Mr. J. A. VAUGHAN: In justice to myself and the other engineers who looked at that rope which Mr. Whittome mentions, I must say that in every case the estimate was made on the external wear only, and this external wear was not evenly distributed.

Mr. A. F. CROSSE: It is with great diffidence that I make any remarks on this interesting paper before gentlemen of your experience. There are only one or two points which I should like to refer to. The part with reference to the effect of nascent hydrogen on steel or iron wire was very in-

teresting, and I think a great deal could be done to examine the effect of nascent hydrogen on steel wire, as to the structural change which is undergone. I think this could be got at in the following way. We all know that a very small proportion or percentage of impurity in a metal affects its malleability to an extreme extent. For instance, a very small proportion of tin or arsenic in gold affects the malleability of that gold. I should look upon a piece of steel wire which contained hydrogen as an alloy of hydrogen and iron, and it would be very difficult to determine a trace of hydrogen chemically. But there is another way in which I believe it could be done. Electrical engineers will tell you that if there is a small amount of impurity in a copper wire it increases its resistance to the electric current. Why should we not use the same method with a rope which has been exposed to dilute acid, and ascertain whether hydrogen had entered into the wire and formed an alloy, and therefore increased the resistance of the wire? I think that could be easily proved, and if so it would be of great value in testing the existence of nascent hydrogen. I have had a great many different mine waters to analyse, and I believe the deeper we get down more acid will be found in the water, owing to the larger surface exposed to the action of decomposition, and the action of acid water on a wire rope is a very important thing indeed. Our ideas on the physical conditions of matter are different from what they were formerly. We used to consider all matter as being divided into gases, liquids and solids, but now we know that nature does not draw such sharp lines of demarcation. For instance, it has been proved that in the case of a disc of gold on which a disc of lead has been placed that after a time the two metals mix together to a certain extent, and so we might find that hydrogen does get right into the metal and alter its structure, and this would prove an interesting field of research for an electrician.

Mr. A. HEYMANN: It gave me great pleasure to listen to this most valuable paper. Mr. Whittome has just forestalled me in what I was going to say, and I can only confirm now that the paper read to-night before you is of an exceptionally great value and importance, as it deals, apart from the technical advantages, with a matter important to humanity, viz., the solution of a problem which means safety to life.

As for criticising the paper I have just heard, I confess it is rather a difficult task—that is, as far as the chemical part is concerned. With most of what the authors say themselves and their conclusions I fully agree, but that part where they bring forward several authorities and argue on the point is very contradictory, and in several instances totally wrong, in my opinion. Unless it is cleared and these points thrashed out I am afraid it will not help us much to the solution of the problem. The last time I had the pleasure of being present at a meeting of this Institute I was greatly impressed by a paper read by Mr. Thomas on wire ropes; so greatly was I impressed that I made up my mind at once to carry out some experiments as soon as time would allow me. I approached Mr. Thomas a few days after, and he supplied me with wire ropes and also procured for me the same "acid" water of which he spoke so much, viz., from the Crown Reef shaft. Some of the results I have obtained, which I will give you presently, will prove that certain arguments of the authorities brought forward by the authors are wrong. As, for instance, when one of them says "it has been proved (?) that though H_2O and O_2 are necessary for rusting, the presence of CO_2 is not necessary," and that "the efficiency of alkali in preventing rust is not due to withdrawal of CO_2, but to the prevention of the formation of H_2O_2." There is nothing simpler than to prove that CO_2 is necessary in addition to H_2O and O_2. It is only necessary to carry out one of the experiments I have made to see for yourself that this argument is wrong and that it *is* essential to have CO_2 to produce rusting and that if you exclude CO_2 no rusting will take place with the agencies of moisture and oxygen alone. I therefore fully agree with the argument of Dr. Moody that the primary action in rusting is the oxidation of ferrous bicarbonate previously formed from CO_2 and iron, also that the theory brought forward by Mr. Dunstan, and supported by Dr. Jowett, *re* the H_2O_2 is, to say the least, totally out of question, especially so where it is known that ferrous bicarbonate destroys hydrogen peroxide.

To come back now to my experiments. I had two different wires: (*a*) crucible plough steel and (*b*) galvanised steel. With

each I performed the same experiments; cut them in pieces of about 6in. and fastened them on a piece of string in a vertical position. At about 6 — 8in. above them a row of bottles was arranged, each bottle provided with drip cocks and clamps so as to be able to adjust the flow of liquid. The bottles were filled (*a*) with ordinary water from the tap, (*b*) with the water from the Crown Reef shaft, and (*c*) with a 10 per cent. solution of sulphuric acid. The last I made on the suggestion of Mr. Thomas, as it was asserted that the Crown Reef water was as "acid," or rather corrosive as a 10 per cent. acid water, if not more. Then I allowed the solutions to drip on the wire, at the rate of 25 drops per minute, for 48 consecutive hours. Some more pieces of wire I distributed as follows:—Immersed in a bottle of (*a*) ordinary water, (*b*) 10 per cent. acid water, and (*c*) Crown Reef water with no corks in bottles; another piece of each in ordinary tap water, previously boiled so as to exclude the air, and, after placing the wire in, covered it, the cork having a calcium tube attached so as to allow air to come in minus CO_2.

The results were as follow:—

The original diameter of the plough crucible wire was ·0615in., of the galvanised ·06in.

Crucible wire, after dripping for 48 hours, plain water reduced to ... ... ·060

Crucible wire, after dripping for 48 hours, Crown Reef water reduced to ... ... ·057

Crucible wire, after dripping for 48 hours, 10 per cent. sulphuric water reduced to ... ... ·029

Galvanised wire, after dripping for 48 hours, plain water reduced to. Not touched

Galvanised wire, after dripping for 48 hours, Crown Reef water reduced to ... ... ·056

Galvanised wire, after dripping for 48 hours, 10 per cent. acid water reduced to ... ... ·042

Crucible steel wire, submerged in bottle not corked, plain water reduced to ... ... ·059

Crucible steel wire, submerged in bottle not corked, Crown Reef water reduced to ... ... ·059

Crucible steel wire, submerged in bottle not corked, 10 per cent. acid water reduced to ... ... All eaten.

Galvanised steel wire, submerged in bottle not corked, plain water reduced to ... ... Hardly any action.

Galvanised steel wire, submerged in bottle not corked, Crown Reef water reduced to ... ... ·0575

Galvanised steel wire, submerged in bottle not corked, 10 per cent. acid water reduced to ... ... ·025

Crucible steel wire submerged in bottle excluding CO_2, plain water reduced to ... ... No action whatever.

Crucible steel wire, submerged in bottle excluding CO_2, Crown Reef water reduced to ... ... Hardly any action.

Galvanised steel, submerged in bottle excluding CO_2, plain water reduced to ... ... No action.

Galvanised steel, submerged in bottle excluding CO_2, Crown Reef water reduced to ... ... No action.

On analysis the Crown Reef water showed free H_2SO_4 20 grains per gal. Total sulphuric oxide $(SO)_3$ 38 grains per gal. I wish to emphasise here that often a needlessly alarming view is taken on seeing a certificate of analysis. It might be stated, for instance, 50 or 60 grains per gal., or even more, sulphuric oxide, or perhaps even expressed as sulphuric acid. I have often heard an exclamation on seeing that, "Oh, how acid!" No, not at all! that does not signify that the water is acid; there might not be any free acid at all! It is the *free* acidity that we have to fear, but this sulphuric acid, which I now always put down as sulphuric oxide in order not to frighten the lay mind, might be and is invariably so combined with a base that it will not do any harm at all to the wire, although it is another thing altogether with the boiler.

This Crown Reef water nevertheless contains some free acid, and even more than at any mine I came across. Notwithstanding this, we see from the experiment that it has very little more corrosive action than plain water; therefore the primary cause of corrosion is *contact with air*. Further, the less contact it has with air, the less the corrosive action, even when absolutely covered with water (when immersed in the bottles), and last, but not least, when the CO_2 is excluded from the air it has no action. We see further from the experiment that no comparison could be drawn between a so-called "acid" water of a mine and a real made up 10 per cent. acid,

and that zinc galvanising of the wires helps very little if at all; on the contrary, once the action starts it will only hasten it.

Since we know what deteriorates our wires, it remains for us to find out now what would be the best means of preventing it—the most effective means. No doubt "tarring" helps a lot, but this has also its disadvantages—the difficulty of examination. In no case would I recommend to use patent lubricants. Mr. Epton is quite right in remarking be careful what you are doing before mixing your lubricants; whereas separately they might not be acid or corrosive, when mixed a chemical combination might set in and free acid liberated. Before doing so find out first the composition of this patent lubricant. But then, when you should find out, which I doubt anybody will give away their "secret," there will not be any need to buy this, you will be able to make yourself for about a hundredth part of its price.

The best remedy I should say would be if we could cover the wires with a non-oxidisable metal and not easily affected by acid, but I am afraid, although possible, it would be found too expensive. Another means I would suggest, if it could be arranged conveniently—a kind of a jacket holding a pound or two of a solution of, say, 1 per cent. of carbonate of soda, through which the rope will have to pass. This would neutralise any free acid the water might contain and would counteract the effects of CO_2.

Mr. JOHN WATSON: I have only to remark on one point, viz., in connection with what has been said on the effect of nascent hydrogen in steel used for such ropes. It is a fact, well known to many chemists, and which I have repeatedly observed myself, in dissolving and reducing iron or iron oxide, with a view to determining the iron by the permanganate or the bichromate method, that, on addition of zinc and increased evolution of hydrogen the iron or iron oxide is dissolved much more quickly by the acid. Beyond this remark I have nothing to add to what has already been said by my brother chemists here this evening.

Mr. J. WOODBURN: I have enjoyed and been greatly interested in the paper we have heard to-night, and when we get the full tables in the *Journal* we shall probably be able to put even a higher value on it. Of course when we enter into the chemical world we have to go more minutely into the subject. From a manager's point of view, the point of standardisation appealed to me very strongly, and if it can be arranged in the future that when a rope is bought there shall be no doubt as to its strength and capacity that will certainly be a great help to those who are using ropes. I think from what we have heard to-night the conclusions arrived at by the authors may be summed up as good lubrication, which will prevent this harmful effect of acid water, and careful examination of ropes.

Regarding the use of winding ropes, it is interesting to note that in Great Britain, where a large amount of mineral is hoisted, the number of accidents from the breaking of winding ropes is very low, and the Government regulations do not make it compulsory to use any form of safety catch.

Mr. WAGER BRADFORD: I can only add my appreciation of this paper. It requires a good deal of study before we can discuss it minutely, but it seems to me to mark a very distinct stage of progress in the practice on these Fields. I can remember when I first came to this country there was somewhat of the same latitude allowed to the users of ropes that Mr. Comstock has attributed to our fellow practitioners in America, and I think it is a hopeful sign that this to a large extent has been changed. Certainly we all desire to see the utmost safety in connection with our winding operations, and the tests being made by our Government laboratory, and the testing apparatus which they have introduced and propose to introduce, are going very far to help us along. So far as the practice in my own country is concerned, I have taken care never to be present when one of those final rope tests mentioned by Mr. Comstock was made. In certain States there is strict Government supervision of mining operations for the protection of life, notably in the coal districts. It is the practice, however, to leave considerably more latitude to mine managers and owners than is done here, and, not being under the Argus eye of the Inspectors of Machinery, they perhaps in some cases overwork their ropes.

TABLE 1.

No. of Test.	Diameter. Wire.	Quantity of Steel. Trade Name.	Ultimate Stress in Mechanical Laboratory.	Mean Breaking Load in lbs.	No. of 180° Bends over $\frac{1}{4}$ in Radius.	Approximate Relational Efficiency under Bending.	No. of Torsions in Lengths of 8 ins.	Approximate Relational Efficiency under Torsion.
	in.		Tons of 2,000 lbs					
104	·049	Improved patent	102·6	387	—	—	62·8	39·46
135	·051	Best plough	120·3	491·49	50·7	165·6	59·4	45·55
136	·051	do.	133·2	544·38	47·5	189·4	51·5	43·73
141	·057	Not quoted	124·5	635·48	20·5	241·2	59·5	52·78
149	·057	Improved plough	124·4	634·75	23·5	225·1	51·5	45·65
86	·060	Not quoted	97·4	551	20·5	179·3	50	36·52
84	·064	Patent	101·3	652	13·7	213·8	46	37·28
72	·072	Not quoted	117·3	955	13	225·9	54·2	57·22
42	·073	do.	132·0	1,105	13	250·8	29·9	36·01
92	·073	Special improved patent plough	135·8	1.137	12·5	263·1	34·1	42·26
113	·073	Not quoted	135·0	1,130	11·4	273·9	38·7	47·67
114	·073	do.	136·4	1,142	11·4	276·7	34	42·32
147	·073	Special improved patent plough	141·4	1,184	11	292·0	30	38·71
58	·075	Not quoted	132·2	1,168	11·2	263·4	34·6	42·88
74	·085	do.	82·6	937	7·86	173·3	38·6	33·88
60	·085	do.	132·3	1,502	11	234·7	28·4	39·92
104	·088	Improved patent	106·7	1,298	9·5	196·7	30	35·21
58	·090	Not quoted	139·2	1,771	7·86	275·9	21·8	34·14
84	·091	Patent	105·2	1,369	8	204·4	32	38·29
102	·092	do.	95·1	1,264	8·3	179·4	26	28·43
97	·093	Not quoted	114·4	1,553·8	8·5	211·0	23	30·59
139	·093	do.	131·5	1·787·07	8·7	239·7	33·2	50·75
10	·098	Best plough	115·0	1.735	10	185·5	35	49·31
11	·098	do.	115·5	1,742	9·8	188·2	28	39·62
13	·099	Not quoted	92·5	1,424	8·8	157·5	21	24·04
33	·099	do.	147·0	2,264	7·4	272·9	20	36·38
83	·099	Extra plough	159·8	2.460	6	329·5	16·7	33·02
160	·100	Not quoted	120·7	1,895·44	9·4	196·8	25·1	37·87
135	·101	Best plough	125·2	2,005·64	9	206·6	34·8	55·01
136	·101	do.	125·3	2,007·21	9	206·8	34	53·78
86	·102	Not quoted	124·8	2,039	7·6	221·9	28	44·55
141	·104	do.	134·1	2.278	7·2	240·3	29·2	50·90
72	·106	do.	131·6	2.323	6·7	239·8	29·9	52·14
134	·107	do.	118·4	2,129·6	6	225·9	26·3	41·65
9	·109	do.	98·5	1,838·35	5·2	198·1	26	34·89
149	·114	Improved plough	120·5	2,460·61	6·57	206·2	26·57	45·62
8	·122	Improved patent crucible	89·6	2,095·8	7·6	133·2	33·8*	41·05
12	·123	Plough	106·9	2,540	6·5	170·5	21*	30·68
1	·128	Crucible	99·7	2.567	4·9	176·0	27·8	44·35

*Torsions in length of 9 ins.

No. of Test.	Diameter of Rope.	Construction of Strand.	Size	g Stress erial. sq. in. er's rate.)	Remarks.
1	2	3		3	
	in.		in.	lbs.	
159	$\frac{5}{8}$	5/1	·030		Soft core wire
8	$1\frac{1}{8}$	6/1	·122	0	Hard core wire
12	$1\frac{1}{8}$	6/1	·123	123	Three strands had a soft core of one wire, and three strands had a cable core of fine wire
9	1	6/1	·109		Core wires partly hard and partly soft
153	1	6/1	·109		Soft core wire
155	1	6/1	·109		Core wires partly hard and partly soft
13	$\frac{7}{8}$	6/1	·099		Hard core wire
10	$\frac{7}{8}$	6/1	·098		do.
156	$\frac{7}{8}$	6·1	098		Solt core wire
145	$\frac{3}{4}$	6·1	·083		Hard core wire
157	$\frac{3}{4}$	6/1	·082		Core wires partly hard and partly soft
135	$\frac{5}{8}$	6/1	·072		Hard core wire
104	$\frac{7}{8}$	7/4/1	·088		Hard core wire. *Calculated from breaking load of strands
149	$1\frac{1}{8}$	7/5/1	·114	3	Soft core wire
151	$1\frac{1}{8}$	7/5/1	·110		do.
87	$1\frac{1}{8}$	7/5/1	·112		do.
136	1	7/5/1	·101		Hard core wire
135	1	7/5/1	·101		do.
154	1	7/5/1/	·100		Soft core wire
150	$1\frac{1}{8}$	8/6/1/	·101		do.
152	$1\frac{1}{8}$	8/6/1/	·103		do.
86	$1\frac{1}{8}$	8/6/1/	·102		do.
58	$1\frac{1}{8}$	10/5/1	·090		Hard core wire
147	$1\frac{1}{8}$	19	·073	145	Soft core wire
113	$1\frac{1}{8}$	19	·073		Hard core wire
114	$1\frac{1}{8}$	19	·073		do.
42	$1\frac{1}{8}$	19	·073	4	do.
92	$1\frac{1}{8}$	19	·073	145	Soft core wire
158	$\frac{3}{4}$	19	·048		Hard core wire
	$1\frac{1}{8}$	19	·083		Soft core wire
	$1\frac{3}{8}$	19	·090		do.
	$\frac{3}{4}$	7	·085		do.
	·716	6/1	·0814		do.
	·716	6/1	·0812		Hard core wire
	·716	12	·0463		do.
	·716	15/9	·0384		do.
	·716	12/6/1/	·0481		Soft core wire
	·756	6/1	·0871		do.
	·756	6/1	·0873		Hard core wire
	$1\frac{1}{8}$	8/6/1	·1046		Soft core wire

en manufactured of soft material.

TABLE III.

Diameter of Rope.	Construction of Strand.	Diameter of Wire.	Quality of Steel.		Mean Breaking Load of Wires.	Breaking Load of Whole Rope.			Loss of Strength of Wires by their being in the form of Rope.
			Trade Name.	Ultimate Stress.		Calculated from Breaking Load of Wires.	Calculated from Breaking Load of Strands.	As Tested.	
in.		in.		tons.	lbs.	tons.	tons.	tons.	per cent.
5/32	12/6/1	·045	Basic	53·13	169	9·63	8·50	8·53	11
5/32	12/6/1	·047	Crucible	89·63	311	17·72	15·50	14·68	17
5/32	12/6/1	·040	Basic galvanised	40·19	101	5·75	5·47	5·12	11
5/32	6/1	·072	Crucible	101·44	826	17·36	14·97	15·65	10
5/32	6/1	·070	Basic galvanised	40·02	308	6·47	6·35	6·09	6
5/32	6/1	·072	Basic	59·68	486	10·21	9·58	9·45	7

TABLE VI.

Ratio. $\frac{D}{\delta}$	Gold Mines.			Coal Mines.		Total.
	Number of Ropes.			Number of Ropes.		
	Flat.	Circular.		Circular.		
	For Rock or Persons.	For Rock or Persons.	For Rock only.	For Rock or Persons.	For Rock only.	
Under 500	—	4	6	4	—	14
500 to 600 ..	—	15	3	—	—	18
600 to 700 ..	2	20	9	6	—	37
700 to 800 ..	4	53	9	6	—	72
800 to 900 ..	—	41	8	4	1	54
900 to 1.000 ..	—	32	12	2	—	46
Over 1.000 ..	2	42	16	—	—	60
Totals	8	207	63	22	1	301

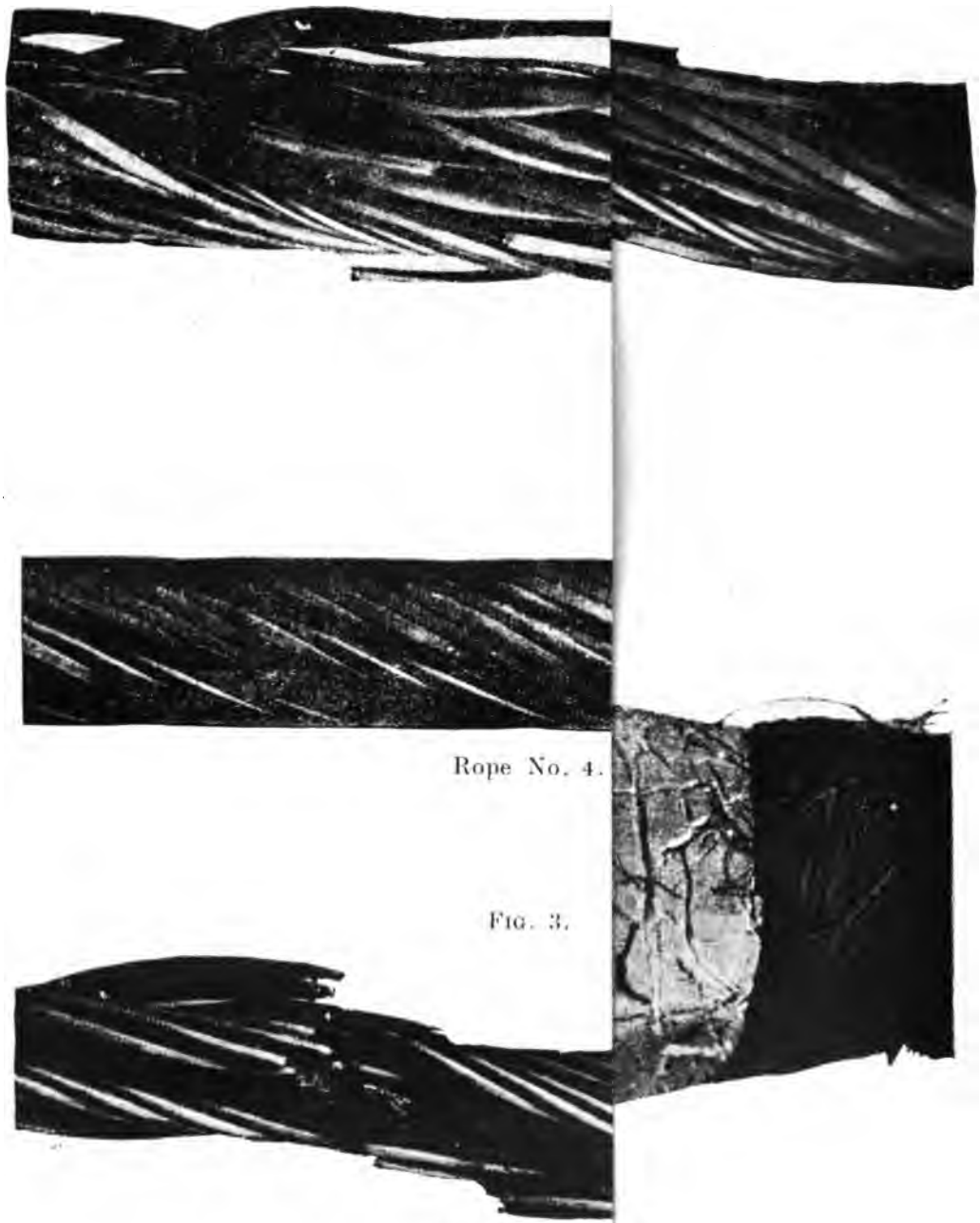
The average values of the ratio are —

For vertical shafts .. .. .. 843·8
For incline shafts 829
For compound shafts 984

TABLE IV.—PARTICULARS *re* ROPE DRESSING USED ON 9 MINES.

Distinguishing No.	Is it put on Hot or Cold ?	How often is it put on ?	Does it run down the Rope when Rope is hanging in Shaft.	Does it Harden ?	Does it leave the Wires when Rope is Wet ?	Composition.	Results of Test in Mines Department Laboratory.	Further Remarks—Mechanical Laboratory.
74	Hot	Once a week	No.	Yes	Yes	Stockholm tar.	Not tested.	
75	Hot	Once a week	Yes	No	Yes	Anti-friction grease and Stockholm tar.	Practically neutral, if anything, a tendency to alkalinity.	Runs off, and does not penetrate rope.
76	Hot	Every 2 weeks	No	Yes, but does not peel off; becomes elastic	No	Unknown.	Quite neutral, and appears to consist solely of vaseline with a little graphite.	Remains oft, penetrates rope, and does not run off.
77	Hot	Every 3 weeks	No	Yes	Yes	Unknown.	Neutral, and appears to consist solely of vaseline with a little graphite.	Remains soft, penetrates rope, and does not run off.
78	Cold	Once a week	Yes	No	Yes	Stockholm tar and raw linseed oil.	Decidedly acid, and on shaking with water gives a strongly acid solution; contains also creosote.	Runs off, and does not penetrate rope.
79	Hot	Once a week	No	Yes	Yes	Unknown.	Slightly acid; contains creosote.	Sets hard, and does not penetrate.
80	Hot	Once a week	No	No	No	Antifriction grease, mineral oil and graphite.	Very faintly alkaline to litmus.	Remains soft, penetrates rope, and does not run off.
81	Hot	Once a week	No	Yes	Yes, after about a week's run in wet shaft	Patent composition with Stockholm tar, tallow and graphite added.	Distinctly acid.	Very thin remains soft penetrates rope, but also has a tendency to run off.
82	Hot	Every 14 days	Slightly	No	No	Unknown.	Practically neutral.	Remains soft, penetrates rope, and does not run off.

No. of Test.	Diameter of Wire.	Elongation in 10 ins	Breaking Load	Before Immersolution. Breaking Load per sq. in.		Loss or gain in strength (apparent).	
						Decrease	Increase.
	in.	in.	lbs	lbs.	tons.	per cent.	per cent.
7/131 B	·093	$\frac{7}{16}$	1,542·				
				215,782·2	107·89	5·0	
				205,970·9	102·99	9·3	
2/131 B	·093	$\frac{13}{32}$	1,586·				
				170,020·3	85·01	27·2	
				181,377·8	90·69	22·4	
3/131 B	·093	$\frac{15}{32}$	1,432·				
				157,977·7	78·99	25·1	
				166,048·8	83·02	21·3	
4/131 B	·094	$\frac{15}{32}$	1,498				
				186,434·0	93·22	13·7	
				191,453·8	95·73	11·3	
5/131 B	·094	$\frac{7}{16}$	1,675				
				205,042·9	102·52	15·0	
				176,761·1	88·38	26·8	
6/131 B	·094	$\frac{13}{32}$	1,719				
				182,991·7	91·50	26·1	
				175,159·8	87·58	29·3	
1/86 B	·102	$\frac{17}{32}$	2,027	256,346·8	128·17		3·3
2/86 B	·102	$\frac{33}{64}$	2,071				
				223,618·3	111·81	11·8	
3/86 B	·103	$\frac{1}{2}$	1,983	229,484·5	114·74	3·6	
4/86 B	·102	$\frac{17}{32}$	2,027				
				228,296·1	114·15	8·0	
5/86 B	·102	$\frac{1}{2}$	2,049	250,420·0	125·21	0·2	
6/86 B	·102	$\frac{17}{32}$	2,027				
				214,760·5	107·38	13·5	
7/86 B	·102	$\frac{1}{2}$	2,093	266,774·2	133·39		4·1
1/124 B	·103	$\frac{1}{2}$	2,226	230,434·0	115·22	13·7	
2/124 B	·103	$\frac{1}{2}$	2,314	226,691·6	113·35	18·4	
3/124 B	·104	$\frac{17}{32}$	2,204	235,243·0	117·62	9·3	
4 124 B	·103	$\frac{17}{32}$	2,204	208,683·3	104·34	21·1	
5 124 B	·104	$\frac{1}{2}$	2,027	209,608·3	104·80	12·2	
6/124 B	·104	$\frac{9}{16}$	2,226	250,553·9	125·28	4·4	
7/124 B	·103	$\frac{31}{64}$	2,204	235,243·0	117·62	11·1	



Rope No. 4.

FIG. 3.

Rope No. 87.

TABLE VII.

FACTORS OF SAFETY IN VERTICAL SHAFTS.

	For Persons only.				For Rock only.			
	No. of Ropes.	Max.	Min.	Mean.	No. of Ropes.	Max.	Min.	Mean.
Including bending stress ..	14	6·37	3·13	5·1	20	8·54	2·56	4·3
Excluding bending stress ..	14	33·31	9·65	15·83	20	31·28	7·17	13·6

ROPES USED FOR PERSONS OR ROCK.

	No. of Ropes.	When used for Persons.			When used for Rock.		
		Max.	Min.	Mean.	Max.	Min.	Mean.
Including bending stress.. ..	98	7·02	2·63	4·7	6·22	2·48	4·4
Excluding bending stress ..	98	120·07	9·26	18·5	72·18	5·54	14·05

FACTORS OF SAFETY IN INCLINE SHAFTS.

	For Persons only.				For Rock only.			
	No. of Ropes.	Max.	Min.	Mean.	No. of Ropes.	Max.	Min.	Mean.
Including bending stress..	3	4·16	3·17	3·61	25	7·22	3·03	4·51
Excluding bending stress..	3	36·88	16·85	22·60	25	24·72	9·48	16·33

ROPES USED FOR PERSONS OR ROCK.

	No. of Ropes.	When used for Persons.			When used for Rock		
		Max.	Min.	Mean.	Max.	Min.	Mean.
Including bending stress ..	76	9·16	2·65	5·08	8·17	2·35	4·46
Excluding bending stress ..	76	57·16	12·09	23·58	27·75	7·48	15·65

FACTORS OF SAFETY IN COMPOUND SHAFTS.

	For Persons only.				For Rock only.			
	No. of Ropes.	Max.	Min.	Mean.	No. of Ropes.	Max.	Min.	Mean.
Including bending stress..	1	4·88	4·88	4·88	16	6·2	2·77	4·44
Excluding bending stress..	1	12·42	12·42	12·42	16	13·94	7·00	9·8

ROPES USED FOR PERSONS OR ROCK.

	No. of Ropes.	When used for Persons.			When used for Rock.		
		Max.	Min.	Mean.	Max.	Min.	Mean.
Including bending stress.. ..	49	7·14	3·61	5·1	5·98	3·31	4·38
Excluding bending stress ..	49	23·28	9·21	15·87	15·97	6·88	10·88

PLATE II.

Rope No. 28 .

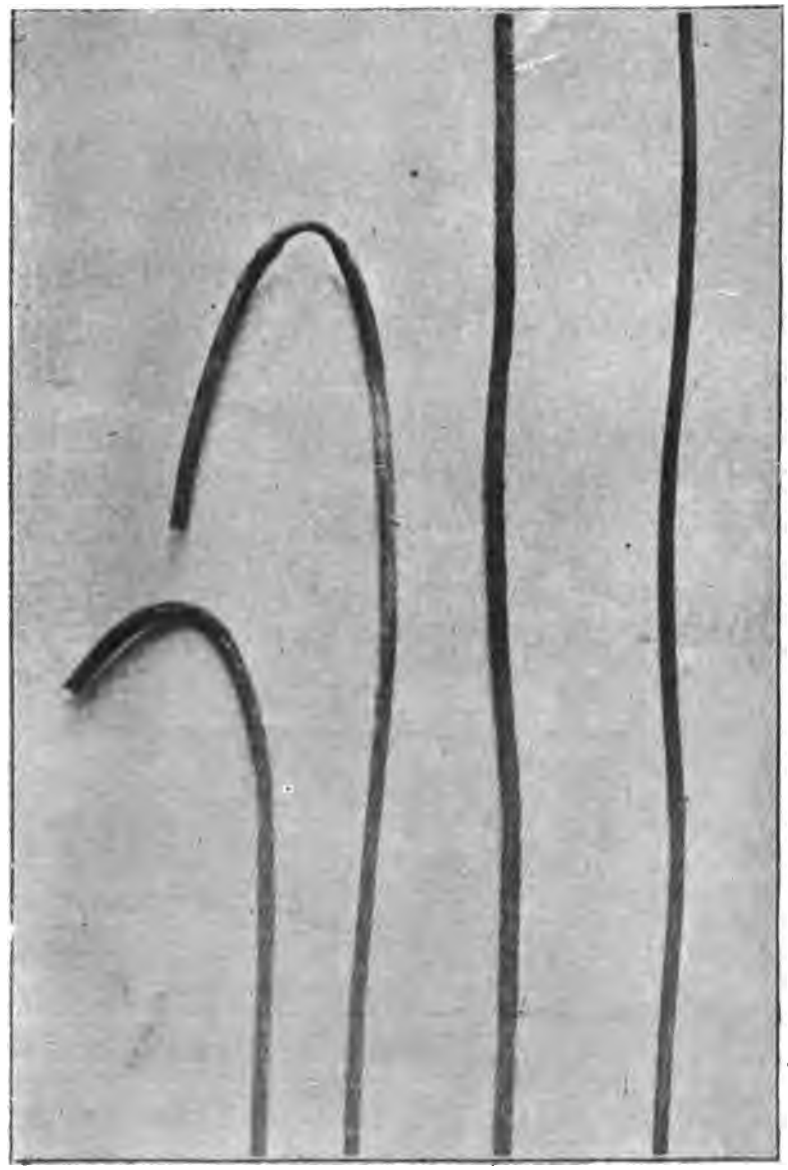


Fig. 3. Rope No. 3/131 A.

Fig. 4.

Fig. 1. Rope No. 6/131 A.

Fig. 2. Rope No. 5/131 A.

PLATE III.

ROPE OPENED FOR EXAMINATION.

ROPE AFTER EXAMINATION.

(Owing to a difficulty experienced in photographing this rope, it required to be opened five times in the same place. This plate shows the rope opened and closed for the fifth time in the same place.)

PLATE IV.

Rope No. 27.

Rope No. 64.

Rope No. 77.

Rope No. 94.

Rope No 95.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. Vaughan and W. Martin Epton (*Members*).

DISCUSSION.

The Hon. Secretary read a contribution by Mr. Schweder, which was as follows:—

Mr. Schweder: I do not think it necessary to specially mention the importance of the paper before us to the whole mining community, but I am quite sure that the managers and resident mechanical engineers on the mines, as well as the other engineers here who have anything to do with hoisting engines, will be very thankful, not alone for the large amount of information the authors have made public, but likewise for the trouble the two gentlemen underwent to follow so closely the latest investigation of Prof. Hrabák, of the Mining School in Przibram, Bohemia.

The latter gentleman had gained already a wide reputation in engineering circles on account of his considered classical investigation on the steam engine, and his latest publication, in 1902, on hoisting ropes seems to have passed without any adverse discussion in Germany and Austria, for I have never found any notes in the respective technical papers to that effect. The conclusions drawn by Prof. Hrabák are, therefore, apparently silently acknowledged as being correct.

The two authors, Messrs. Vaughan and Epton, are giving him due credit for his work, too, and I need not apologise for dealing in some places more closely with his investigations.

The paper before us is, I think, simply splendid, and I feel there is not very much in it to criticise or to improve upon.

The question of factor of safety has been touched upon already by our President, and I think that it will not be very difficult to come to agree with a certain and compulsory figure if we only feel sure that we can compute to a nicety the breaking strength of a rope when new, and, what is just as important, when in use for some time.

Regarding the remark of the authors that the sacrifice of toughness and ductility is not so very great when choosing a higher tensile strength of wire, I beg to quote here the opinion of the Head Engineer at the Przibram shafts, where they are hoisting for many years from a depth of over 3,000 ft. He told me that he does not like to take wires over 125 to 130 tons tensile strength, on account of their brittleness and their greater liability to corrode.

But this, of course, is only one man's opinion, and he might not have tried very many makes of rope.

When the authors write that it is the custom on the Rand to secure the ropes on the outside, as reasons might be mentioned here, that the clutches are nearly always outside, and if the distance from drum middle to drum middle is larger than the distance between two shaft compartments, we will obtain two clean rope layers if the drum shells are grooved.

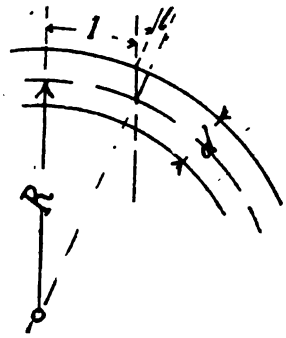
Re wire cores, Prof. Hrabák is indeed very little in favour of such construction, and he states that the wire cores alone carry 62½ per cent. of the full load, under the assumption, however, that all the stresses are within the limits of elasticity. He believes that the core does not break, because it is gripped by the outer wires, but I think that the core wire is simply outstretched by the first load application, and when the rope and the wires in the strands creep back, the wire core, which has to go back a long distance, will find sufficient room to buckle out between the spirally laid wires.

The authors will have, perhaps, the chance to state occasionally whether the modulus of elasticity is indeed appreciably altered in core wires after being in use.

Considering that the bending stress in a rope might be so great that it might under unfavourable conditions (when the pulleys and drums are very small, and the wires in the rope are chosen too large) be nearly as much as the uniform stress in the straight rope under full load, I believe it is worth while to stop a little at Prof. Hrabák's theory regarding these stresses.

About the year 1865 Prof. Reuleaux tried to solve the problem in the following manner, that he took each wire in a wire rope, evenly bent round the radius R of the sheave or drum, and he puts the elongation l of the wire length 1 over $\frac{d}{2}$ = wire length $\frac{1}{R}$ or $\frac{l}{\frac{d}{2}} = \frac{1}{R}$. The elongation l of the wire length 1 of the cross-section $1 = l =$ stress

per sq. unit, divided by the modulus of elasticity E.



It follows thus : $\frac{\text{stress}}{E} = \frac{d}{2R}$ or stress due to bending $= \frac{Ed}{D}$. This theory caused quite a consternation amongst the wire rope users, but it was soon found out that the formula gave too high values for this stress, and the experts, including Prof. Hrabák, were sure that the theory wanted a modification, which, however, only lately—in 1901—Prof. Hrabák believed he had struck. He first collected all statements of tests made, in order to ascertain the elasticity of new wire ropes, and he found that the moduli of elasticity E of all the ropes were pretty near alike, and averaged 0·36 the modulus of elasticity of the material E_0 the wires were made of.

If the stress now were computed acccording to Reuleaux's formula, stress $= \frac{Ed}{D} = \frac{0{\cdot}36E_0d}{D}$ this would give too low values, because, Prof. Hrabák explains, we have not to deal with a bending of the total rope, but we have to view the bending of the various wires in it, each forming a double spiral in the lay of the rope. The fractional elongation in the wire must be smaller than if the wires in the rope were all straight, and the modulus of elasticity (as the reciprocal value of this fractional elongation) of the wire in the rope called E_1 must be greater than E of the rope. The authors explain this very nicely by means of a sketch, and state the figures $E_1 = 1{\cdot}106E$ for strands $= 1{\cdot}222E$ for ropes, and $= 1{\cdot}351E$ for cables.

Prof. Hrabák gives then the stress in the outside fibres of each (spirally bent) wire due to the bending round the radius $R = s =$ $\frac{E_1d}{D}$ and for the ropes which are concerning us mostly $s = 1{\cdot}222 \times 0{\cdot}36E_0\frac{d}{D} = 0{\cdot}44\frac{E_0d}{D}$.

Messrs. Vaughan and Epton here draw attention to the fact that short pieces of wire in the ordinary lay of rope are parallel with the axis of the rope, but I think that if the wires were computed accordingly we would come back to Prof. Reuleaux's formula, and this latter would be correct if the wires were resting on a rigid support.

This is, however, not so, or the modulus E could not be so much decreased as $0.36E_0$, and I think that the excessive strain on the crown of the wires, where their angle with the axis of the rope equals α, is released by the part of the wire which forms an angle of 2ω with the rope axis.

There is surely a differential movement of all the wires in the rope when the latter is bent, but this sliding of the wires upon each other need not be so very great, as the bending of all wires is in the same direction.

Dealing then with ropes in use in the mines, Prof. Hrabák found that the modulus of elasticity greatly increased until in time it remained for a good while almost constant, and averaging $E = 0{\cdot}72E_0$ instead of $0{\cdot}36E_0$, as found with new ropes. The figures were arrived at by loading the cages with men in many vertical deep level shafts, and noting the elongation of the rope thereby. In a 3,000 ft. shaft the elongation amounts to about 1 in. per man, and can thus be measured reliably and accurately. Prof. Hrabák corroborated thereby his views regarding the wire cores, and found that the elasticity is 22 per cent. less than in wire ropes with hemp cores.

He found also that the Lang lay ropes are 10 per cent. more flexible than the ordinary lay of ropes, and thus he recommends the former construction if short bends are not avoidable. Speaking of Lang lay, I cannot help mentioning a passage in Prof. Hrabák's book, where he complains that the rope construction which is called in England Lang lay has been introduced as far back as 1834 by a mining man in the Harz Mountains, called Albrecht, and that, nevertheless, this construction is patented in England and claimed to be manufactured first by Messrs. G. Cradock & Co., as suggested by Mr. Lang. This Lang lay is

called in Germany and Austria Albert lay, after this first designer. But this only *en passant*.

In order to prove or disapprove the correctness of the Hrabák theory, I believe it would be a grand thing if the authors could make it possible to test pieces of ropes or strands, at least, while they are submitted to bending stresses also, in order to see whether the practical results coincide with theoretical conclusions. Tests of that kind would, indeed, excite the greatest interest all over the world, and would be of a special value to this country as long as we have to import hoisting engines and like to choose the drums as small as possible for that reason.

Coming to the end, I only beg to differ from the authors when they state, under reduced strength of ropes, due to wear, that the rope has lost 25 per cent. of its strength, when 25 per cent. of the wire cross area has been worn away.

This stands good only if the bending stress is not taken into consideration, but if we take the resistance of the rope as composed of resistance against the load and against the bending force, the former is undoubtedly reduced in the quoted instance by 25 per cent., but the latter remains the same (if the higher modulus of elasticity in used ropes is not taken in calculation), and the rope under this view must have lost less than 25 per cent. of its original strength. The real loss can, of course, only be made out individually for each instance.

Prof. HELE-SHAW, in response to the President's invitation, said: I had hoped you would not call on me to speak to-night. I really came down partly to listen to the discussion and partly out of respect to the authors. When you remove to a building where the use of the lantern will be permitted, I should like to show you the micrographic pictures referred to in the paper, taken from wires in a rope which failed and caused great loss of life. These pictures reveal in an extraordinary manner the changes which had taken place in the steel. I hope, in connection with the valuable work which is being done on the Rand by the Mines Department in testing wire ropes that it will be usual to have such microphotographic pictures prepared. I have been doing some of this work lately, and I am convinced that it should be undertaken in every experimental testing laboratory. There has been a great deal of investigation on specimens which have been prepared from new material, but there has been very little work of the above kind on specimens which have been in actual use. The difference in the above case between the original wire and the sections that failed is such as to make it doubtful whether it was due simply to "fatigue" because there is no doubt the part that is gone in this wire was not merely being continually bent, but was also very thin and corroded. Was the change shown in the photographs caused by mechanical action or due to the action of acid water in the body of the material? Engineers are not accustomed to regard steel as being permeable in this way, but we know that even denser metals are permeable, and it might be possible for a liquid to penetrate beneath the outer skin of the steel itself. I look forward to experiments on sections of metal to throw a great deal of light upon failures that occur from time to time. At any rate, it is the opinion of all who have to do with the testing of steel that little is known about its nature. Chemical analysis reveals to us certain constituents, but it does not show us the million ways in which the particles of the different constituents are combined, and there is no other way of finding this out than by this process of photography of very minute sections. From experiments of that nature, combined with the tests, we shall probably greatly increase our knowledge. As to the paper generally, after I have had time to study the tables and figures I should like to make a further contribution on the subject.

The PRESIDENT: We shall be most happy to hear you on any other occasion.

Mr. J. R. COWELL: In making a few remarks on the able paper presented to this Institute by Messrs. Vaughan and Epton, I must express my regret at not having been able to be present at last meeting, when it was presented to you by the authors, but have since read it with great interest. This paper, as presented to you, bears evidence of great care and thought in its preparation, and of research work into the particular conditions prevailing here, which should be of great use both to the users and manufacturers of wire ropes.

The authors refer very early in their paper to the necessity of selecting ropes particularly for the work they are required to perform. Too much stress cannot be laid on this point. It has, in the past, been quite usual for purchasers to simply ask for a rope of a given diameter, and with

a breaking strain of so many tons per sq. in., without any further indication of the conditions under which it was to work. The bending stresses, due to small drums and sheave pulleys, were quite unknown to the manufacturers, and frequently a rope has been condemned as of bad manufacture, when had the makers known the conditions under which the rope had to work, they would have recommended a rope of entirely different construction, or declined to give any guarantee whatever. I am glad to note, however, that now it is becoming common practice to give a full diagram of the lead of the rope, together with diameters of all pulleys and drums and conditions of working, and to ask the manufacturers to specify the rope they would recommend for the work.

There is, further, no doubt that inexperienced agents, anxious to make business, will take an order and supply a rope without any practical knowledge of what they are doing, and trust to providence that it will come out all right. The success of a rope manufacturer can only be due in the end to the care taken to supply suitable ropes for the work required, and it is important that the user specifies these conditions to the fullest possible extent.

Respecting high grade steel wires, in col. 9, Table 1 are given a number of tests of steel wires of over 135 tons per sq. in. breaking strain. I notice that the number of twists in lengths of 8in. is, however, very low. Ropes made of wire of 140 tons per sq. in. section, with a torsional test of not less than 55 turns in 8in., can quite well be supplied.

The torsional test is of the greatest importance in ropes, not because the ropes are subjected to any great torsional stresses, but because it is essential that the ductility, which is shown by the torsional test, is maintained. It is very easy to make wire hard and get almost any breaking strain you like at the expense of ductility. To make sure of getting a good rope, the ductility or torsion test should always be insisted on.

There does not, however, appear to be any demand here yet for such high grades of steel in wire ropes. The authors state that they have found the length of 8in. the most favourable for torsion. The standard, however, is that the length of piece for torsion shall be 100 times the diameter of the wire under test, which gives 8in. for wires of No. 14 gauge, or ·08in. diameter.

As to the core in strand of rope, this should not be considered in calculating the strength of rope, whether of wire or hemp.

Respecting the Mining Regulations, it appears highly desirable that absolute uniformity should exist as to the rating of ropes. More confusion perhaps is caused here than in the British Collieries, owing to the standard rating of British manufacturers being expressed in tons of 2,240 lbs., whilst here it is customary to use the ton of 2,000lbs. It would, however, appear a matter to be dealt with by the Government Mining Engineer's Department, who could require such forms of certificates as would practically compel the rope manufacturers to fall into line.

With reference to the method of preparation of test samples of ropes. This appears to be open to question.

The method of tinning the wires and fastening them in a conical mould is, I believe, quite sound with respect to ropes of six strands of six or seven wires per strand. In ropes made of six strands of eighteen or nineteen wires per strand, the wires being all of the same diameter, it is also probably correct. In other compound ropes, where the inner wires of the strand are of a different diameter from the outer wires, it is not accurate, as owing to the different diameter of wires the lay is at a different pitch, so that some wires get more strain than others. On ropes of six strands of twenty-four wires, thirty wires, or thirty-seven wires, a very considerable error is made. In one particular instance known to the writer, a rope of six strands of thirty-seven wires per strand was tested by this method. The breaking strain of the rope was calculated for 39 tons of 2,240lbs. Testing the rope by the method of tinning, and separating the wires and using a conical seal, the rope broke under a load of 35 tons. The same rope was then tested by a different method of gripping the ends, and the rope broke at 39.1 tons. It would be interesting if the authors would test another method of fastening the ends of the ropes, and communicate the results obtained by them to this Institute at a later date.

As to the amount of deduction to be made on account of the laid-up efficiency in ropes,

the authors give 10 per cent. as a reasonable figure. I should be glad to know if this is 10 per cent. off the total sectional area of wires in the rope, or excluding the central wires of strands.

Re internal frictional area. The authors give as one of the causes "badly trimmed joints of the wire." This is better obviated by having the wire without any joints, brazed or otherwise. Ropes of 1¼in. diameter can be made in lengths of 6,000ft. without a single joint, and the cost of such is not much increased.

Concerning the experiments carried out on testing wires after treatment by sulphuric acid, the fact that the action of the acid was much more violent at the bends than at other positions is particularly interesting. This reminds me of the problem in which it is desired to find out what becomes of the energy expended in compressing a spiral spring if that spring is dissolved by acid whilst still under compression. Doubtless, the same thing occurs. I should be particularly interested to know if the authors could make more experiments to see whether there is any noticeable increase of corrosion if the wires are under tension, as it is quite conceivable that this is so, and that corrosion of ropes is much more rapid from the action of acid water when under the stress of working than when at rest.

As to the remarks on the bending of the wires when laid in a rope being less severe than when laid straight, a very good illustration of this can be seen by taking a piece of large diameter ordinary lay Manilla rope, when the various effects caused by bending can be easily followed.

Mr. LEUPOLD: I had intended to make a few remarks especially with reference to the modulus of elasticity, as it is altered by the laying of wire, but I find I have been forestalled by Mr. Schweder's contribution. I would, however, seize the opportunity of calling special attention to the great importance of this point, because in order to arrive at a correct determination of the bending stresses, which we know form a large part of the total stresses, we must first be sure about the modulus of elasticity, and the question whether the wires in that part of their course, where they are parallel to the rope axis, are to be treated as single wires laid on a drum, or in what degree they are relieved from the increased stress must be determined. I am writing now to Prof. Hrabak about this, and I hope he will be able to give his views on the matter. Should the discussion be closed before I get a reply, I shall be happy to contribute a short note to the *Journal*.

Mr. VAUGHAN: Arising out of Mr. Schweder's contribution—I know that Mr. Leupold has studied Prof. Hrabak's book pretty fully, and that he is also able to understand the language in which it is written. I should, therefore, like to ask him one question. Referring to the modulus of elasticity, has Prof. Hrabak ever practically tested the elasticity of *old* ropes hanging in shafts? I know he has done this for *new* ropes. I want to know whether the value he deduces from theory for the modulus of old ropes is corroborated by practical experiments on old ropes hanging in shafts.

Mr. LEUPOLD: I do not now remember all the figures and factors which Prof. Hrabak has deduced from practical experiments. I also do not remember whether they differ very much in ropes which have been used for some time. On the whole, I gather that the practical determination of the modulus of elasticity in a rope, as against that in a single wire, corresponds very nearly with the theoretical deductions which he had arrived at.

Mr. R. WHYTE: I thank you for the opportunity of making a few remarks on the able and interesting paper now under discussion. To me, a comparative stranger, the local conditions under which many ropes work are not sufficiently well known to let me express an opinion on a number of points mentioned in the paper. There are, however, one or two matters I might refer to, and which might interest the meeting.

I hold rather strong views; for example, on the advantages of galvanised ropes for upcast shafts, where foul air or other corrosive elements are met with, or for wet shafts. It seems to have been clearly demonstrated that galvanising is not a success in Crown Deep water, but all the mines are not Crown Deeps, and there are, I feel sure, many others at which the water is used for boilers, etc., where galvanised ropes should, in my opinion, be given a fair trial.

With regard to Mr. Biggart's tests. These I happen to know something about. They were carried out during the building of the

Forth Bridge, where a number of elevators for hoisting men and material were in operation, and the ropes used were, as a rule, galvanised crucible steel 6/24 construction. This, I need hardly say, is a different make from any used on the mines, unless for block and tackle work. How far these tests might have differed, had the usual type of mining rope been experimented upon, I am not in a position to say, but, individually, you have some chance of getting data from your driving or transmission of power ropes.

Messrs. Vaughan and Epton refer to wire being used on the reef, drawn to a breaking strain of 160 tons per sq. in., and it would be interesting to know how a rope of this high strain lasted as compared, say, with those of 130 tons. I quite agree with the writers as to the advisability of re-capping ropes every two or three months, but I cannot see how it is possible to open the strands of any rope for the purpose of examining it for internal wear or corrosion without running considerable risk of doing it injury, more particularly if the rope has been at work for some time, and the strands have become to some extent locked.

The question of standardising wire ropes is one presently engaging some attention, but without knowing more of the Committee's intentions as to the lines on which they purpose attaining their object, I would prefer leaving that subject alone, with this one remark, that generally speaking managers and engineers know pretty well what standard of rope suits their particular conditions.

THE PRESIDENT: You stated, Mr. Whyte, that you did not believe in opening up the strands in the rope. Do you mean the rope itself, or individual strands?

Mr. WHYTE: When the rope is flattened down by wear I think it is an exceedingly risky thing to open the rope. You cannot open the strands.

Mr. W. MARTIN EPTON: When Mr. Whyte said 145 tons, did he mean long or short tons?

Mr. WHYTE: Short tons.

MR. TREGASKIS: There are just two points which I should like to mention. In the first place I should like to call attention to the difficulties which we may get into by allowing the rope to remain underground in cases where there is no winding at night time. If there is acid water dropping on to the rope corrosion may be local, and is very easy to be overlooked. The difficulty may be avoided by taking both skips out of the shaft when shutting down in the evening. The other point is in connection with the rule the authors gave us for measuring the wear on the individual wires by measuring the outside diameter of the rope. Sometimes a rope wears flat, and in that case calculations based on the measurement taken round the outside of the rope does not give the real breaking strength. When the wear is on one side of the rope it may very easily break, and yet the total amount of wire gone is not very great. I quite agree with Mr. Heather that when a particular wire in a rope is worn somewhat more than half, the worn surface still shows the full width of its original diameter, so that waiting until the wire that is most worn shows less than the diameter of the wire may lead to considerable trouble. The matter of judging the amount of strength left in a rope by noticing the amount of wear on the wires outside can only be estimated with any degree of accuracy by getting the ropes more frequently tested by the Government officials. If the Government would only reduce their present charges for testing, it would tend very much to the safety of ropes on these fields. We could then send our ropes in and get them tested much more frequently than at present.

Mr. F. W. DUQUEMIN: With reference to to Mr. Whyte's remarks about breaking strains, I rather sympathise with him. My firm send me out the breaking strain of every rope, and I have considerable difficulty in persuading intending purchasers that these breaking strains are the results of actual tests, and not necessarily the aggregate of the wires.

Specifications of ropes are passed to me to tender from. These give both the breaking strain of the wire per sq. in. and the breaking strain of the rope, but I have frequently found that these two particulars do not agree by quite 10 per cent. I mean, of course, when the latter particular is to be the result of an actual test of a piece cut from the completed rope. When I raise this question with the purchaser he invariably replies—"We must have the breaking strain," and the result is that I have to put in a rope with a much higher tensile strength of steel per sq. in. than is

really intended by the user, and which may not work as well as would a lesser one.

Referring to a 6,000ft. 1¼in. diameter rope, made of wires without brazes from end to end, I do not know what construction of rope the last speaker referred to, as obviously a rope of 6 x 19 construction could be made in this way to a much greater length than could a 6 x 7. With a view of supplying such ropes, I inquired of my firm some time ago. I do not remember the actual figures at present, but I feel sure that a 6,000ft. 1¼in. diameter 6 x 7 could not be made without at least one braze in each wire. The largest billet from which wire can be drawn is 200lbs., which would give about 180lbs. of wire.

I have heard of a rule out here on these fields with regard to the construction of ropes for certain sized sheaves and drums, something about 1,000 times the diameter of the largest wire in the rope being the minimum diameter of the sheave or drum for good practice. This rule does not seem to take into consideration the tensile stress of the steel which is used, and I think that this should be an important consideration, as with a steel of, say, 90 tons, you should be able to use far smaller sheaves than with a steel of say, 125 tons. The bending stress for all tempers of wire would, of course, be the same, but the resulting life of the rope would surely be quite different. I do not know whose rule this is, but possibly someone here may be able to tell us.

Mr. VAUGHAN: There is no Government rule with regard to the diameter of the wire in the rope and the diameter of the sheave it is intended for, except that it is provided that the wire shall be suitable for use with the sheaves and drums fitted. In some other countries they have more definite rules. In Austria, I believe, the rule is 1:1,200. Obviously, if you have a rope to use, these considerations are settled by the statical factor of safety you adopt. We know something now about the bending stress, and dead load is given, and if you take it out in dead load you cannot take it out in bending stress. Working to a higher statical factor of safety you can afford to allow more bending stress. For these reasons I fancy we have never suggested anything like a fixed rule for the ratio $\frac{D}{\delta}$ I should think, myself, that anything below a ratio of 1:800 is bad.

Mr. LEUPOLD: I would like to ask Mr. Whyte one or two questions about galvanising wires. Has it not been found that galvanising reduces the tensile strength of the wire? I suppose this may be due to an alloy being formed at the contact between the zinc and the iron, which would, of course, deteriorate the steel in the wire, and thus reduce the total tensile strength of that wire. In any case, it seems to me from what I have read about it that the tensile strength is reduced by galvanising, and therefore the rope must be made heavier than it would be made of steel, only quite apart from the additional weight of the zinc. Also I would like to ask if the zinc is not abraded on the surface of the rope very soon after the rope is put in use, and if the chemical corrosion is not increased on account of that? It seems after the zinc has been worn off in places that the rope corrodes very rapidly.

Mr. WHYTE: There is no doubt that when you come to galvanised ropes, especially in plough steel, it is advisable to keep down the strain. I should not like to galvanise a 145 ton wire and guarantee it to be so good as plain wire. The torsion would not be so good. In galvanising improved plough steel wire, we always recommend that the strain be kept down, otherwise the torsion will be deficient. As regards crucible steel, we find there is very little deterioration in the material. We get, practically, as good mechanical tests from galvanised crucible steel as from plain. If you lost both in strain and torsion by galvanising, then the game would not be worth the candle; but we do not find it so. With regard to the other point, as to the wearing of the crown of the strand and consequently the galvanising, that, of course, is quite true, but we claim if you keep the inside of your rope intact and free from corrosion, you have gained a very considerable point. And that is what I claim for galvanising at Home. I have a sample, which I purpose putting on the table at the next meeting, cut from a galvanised ⅞in. diameter crucible steel rope, which had been at work as a band or strap for two years and four months at an important colliery, in Scotland. Previously, nine months was the average

life got from ⅜in. diameter plain crucible ropes, and when these were taken off, the signs of corrosion were so unmistakable that galvanising was recommended without hesitation, and the results have entirely justified the change. Of course, the larger rope has doubtless something to do with the increased life, but in my opinion the galvanising has much more, for while the rope is pitted on the crown of the strands, where the friction has worn off the galvanising, the inner portions of the strand are in splendid condition, and this must have materially helped its life.

Mr. MAYHEW: Might I ask if Mr. Whyte can give us the chemical analysis of the water in which that rope was running, whether sulphuric acid or sulphate of iron?

Mr. WHYTE: That I cannot give.

Mr. VAUGHAN: With regard to the question of galvanising ropes, there was a very interesting paper read in October, 1897, by Mr. Thomas Lees, in the Federated Institution of Mining Engineers, an extract from which I will read. From the discussion which it brought out, galvanised ropes seemed very suitable for English colliery work.

EXTRACT FROM MR. LEES' PAPER.

"The writer has frequently been asked whether galvanising does not seriously injure the wires, but his experience has fully proved that practically no deterioration takes place if the galvanising has been properly done. Some prejudice has existed in the past against the use of galvanised wire for ropes amongst mining engineers, but it must have been only on the part of those who have had no experience in their use. The writer was informed by a well-known firm of rope makers that it was as important that the galvanising should be done carefully and by the best process as that the wire itself should be properly drawn, and that in either process, if carelessly or improperly done, the wire might be spoilt. The following tests were made by the writer with new wires supplied by the makers of winding ropes, the wires being of the same quality and thickness (improved patent crucible steel, and No. 10 Birmingham wire gauge), but one portion plain and the other galvanised; the object of the tests being to find out what effect, if any, the galvanising had on the strength of the wires:—

Description of Wire.	Torsion in 8 ins. Revolutions.	Breaking Strain lbs.	Bends at Right Angles over 180 degs.
Plain	24	2,310	7
do.	32	2,200	8
do.	25	2,366	7
do.	33	2,086	8
do.	23	2,190	7
do.	27	2,142	7
Mean	27	2,215	7
Galvanised	26	2,142	7
do.	24	2,048	8
do.	29	2,240	7
do.	24	2,360	7
do.	23	2,254	7
do.	27	2,086	8
Mean	25	2.178	7

It will be seen from the foregoing table that the falling off in strength in the galvanised wire is so slight as to be practically of little importance, and no uneasiness need be felt on that score when ordering a galvanised rope.

The extra cost of galvanising a winding rope 4½ ins. in circumference is about 5s. per cwt. The writer's meaning will be made clear from the following example:— Suppose a crucible steel wire rope, 500 yards long, 4½ins. in circumference, weighing about 45 cwts., and costing 35s. per cwt., made of plain wires, the total cost will be £78 15s., and the rope will probably last two years, thus costing £39 7s. 6d. per year. A galvanised wire rope of the same strength at 40s. per cwt. will cost £90, and may safely be assumed to last six months longer than the rope with plain wires, and would in such case cost £36 per annum, showing a saving of £3 7s. 6d. per annum, or £8 8s. 9d. per rope. This is a very low estimate as to the economy of using galvanised wire ropes, as their life might be lengthened 50 per cent. and upwards."

Mr. Whyte in a way attacks the method we propose of examining wire ropes. One eminent rope manufacturer told us that no damage would accrue to the rope from the method we suggested. I should like to ask Mr. Whyte if he has ever known damage to accrue to a rope from the modified untwisting we suggest?

Mr. WHYTE: I admit it is a different matter to open up a new rope than to open up an old one. The photographs I have here would seem to show that the operation was performed on a new rope. I cannot say that I have ever seen a worn rope opened out, but I feel positive that if a worn rope should meet with any kind of accident, which in any shape or form would dislodge the strands, it would be taken off right away.

Mr. EPTON: We opened the rope five times in the same place, and it has not been forced back in any shape or form.

Mr. J. BERRY: Mr. Whyte's remarks would make it appear that we are not acquainted with galvanised hauling ropes on these fields. I have just taken off a 1¼in. diameter hauling rope, 2,400ft. long, after twelve months' work. The rope had the appearance of a new one (except a slight wear) when taken off, and no appearance of corrosion whatever either inside or outside.

The PRESIDENT (Mr. A. C. Whittome): Why was the rope taken off?

Mr. BERRY: It was taken off because it had been on twelve months, and we decided to increase the safety factor.

Mr. DOCHARTY: With reference to the length of wire that can be obtained in making a rope, in my experience I have found that brazes and other forms of joints of wires give considerable trouble. Some time ago I approached a representative of one of the wire rope makers here, and got him to investigate the matter. He wrote Home, and the rope manufacturers stated that although it was not impossible to get as long a wire as we were asking for, 5,500ft., they distinctly stated that it was possible for the structure of the wire to be altered to a considerable degree, owing to it being drawn to such a length, and they stated they could not guarantee the strength of wires over 2,000ft. that they could of those under 2,000ft. It would be interesting to know if we could get these 6,000ft. wires as mentioned. With reference to galvanising wire ropes, Mr. Whyte made rather a strong point. He said that if we could see the deterioration of a wire, and know the outside was the worst part of it, we would feel very safe. I still think that the fact of the zinc remaining intact is a very good guarantee that the internal condition of the rope is perfectly safe, and from what I have seen I think galvanising is a very advisable thing.

Mr. BERRY: I might mention that Messrs. Vaughan and Epton have a piece of the galvanised rope I mentioned, and it is being tested at the present time. I hope they will include in their reply the results of the test, and the conclusions they arrive at as compared with ordinary ropes.

Mr. DOCHARTY: Messrs. Vaughan and Epton have shown us several photographs of ropes opened up that have gone back very well. Perhaps I can remind them of a rope opened up on the City and Suburban which would not go back, and I would like to back up Mr. Whyte's statement that it is a questionable practice. It might be that the ropes which showed good results had never been subjected to a good test or strain, but I certainly think it is a very questionable practice to open up a rope which had been put to a severe strain.

Mr. EPTON: With regard to that rope, Mr. Docharty considered that it was so bad that he had decided to take it off. I think I am right in saying that the rope we have now opened up five times had previously been working for fifteen months, and I think it had been in regular use for that length of time. I tested the galvanised rope to-day, and I looked at it very carefully, and I could not see any wear at all on the wires.

The PRESIDENT: When you put in a record of the test of that rope you might give the details of the life of the rope for the sake of comparison, and possibly also you might be able to persuade one of the mines to carry out a series of opening up tests.

Mr. J. A. MILLS: Since the War we had occasion to put in two new ropes on 6ft. drums, 6ft. headgear pulleys, and compound shaft. One rope was supplied by Mr. Mayhew, and the other (galvanised) was supplied by Mr. Whyte. The non-galvanised rope ran for two years and two months, and Mr. Whyte's rope ran for two years and five months. The results were slightly in favour of the galvanised rope. In cutting the rope the galvanising in all cases was intact.

Mr. LEUPOLD: With regard to Mr. Duquemin's remarks about the relation between the diameter of the largest wire in the rope and the least diameter of the pulley admissible, I think

we are far enough advanced now to discard rule of thumb in this matter, and when a rope for a deep level shaft—where weight comes into consideration—is contemplated, both the bending stresses and the stresses due to dead weight will be calculated. We have come to a point now when users of ropes will no longer say they require a ¾in. rope to carry a certain weight, and let that be the only indication to the maker; but as the stresses in such calculation are apportioned between dead weight and bending stress, the drum in one case may be of such a diameter that the bending stress is only one-fifth of the dead weight stress, and in another case the bending stress may form one-third or more of the dead weight stress. What I should like to bring forward is to ask if it would be possible to modify the law, and allow the factor of safety to be altered so as to include the bending stress, and not be based on the dead weight stress only, as the total stress is the sum of both. A rope which runs over a 6ft. sheave is strained much more considerably than if it were running over a 10ft. sheave, and the factor of safety is quite different in the two cases. As we are advancing in science, I think the law ought to be altered accordingly.

Mr. J. R. COWELL: As to the possibility of making ropes in lengths of 6,000ft. without any joints in the wires, the figures which I gave are based on a compound make of rope, i.e., 6/13, 6/16 or 6/19, as the remarks were made specially with respect to internal friction in ropes. As to the limit to which makers will guarantee wires in one length, they will do this up to 6,000ft., provided the finished wire does not exceed 180lbs. in weight, as this is the maximum weight that can be satisfactorily manufactured in that class of steel. My authority is that of a good rope maker, and he said that he would guarantee ropes under these conditions to be uniform and absolutely good up to that weight.

The PRESIDENT: Can Mr. Docharty give us details of the rope he inquired for?

Mr. DOCHARTY: The length was 3,500 feet, construction 6/7, diameter 1¼in., and they distinctly said they could not guarantee the wires in one length.

Mr. VAUGHAN: There is one matter which was alluded to at last meeting, and which might be disposed of at once. Mr. Woodburn said:—"Regarding the use of winding ropes, it is interesting to note that in Great Britain, where a large amount of mineral is hoisted, the number of accidents from the breaking of winding ropes is very low, and the Government regulations do not make it compulsory to use any form of safety catch." This might lead one to suppose that winding ropes do not break in England, which, of course, is not a fact. In support of this statement I will submit to you an extract from the "United Kingdom Mines and Quarries Statistics" for the year 1902.

Table 63.

Fatal Accidents caused by Ropes and Chains breaking at all Mines under the Coal and Metalliferous Mines Regulations Acts during the years 1893-1902.

Year.	Ropes or Chains Breaking.				Total.	
	In Shafts.		On Inclined or Engine Planes or in Haulage.			
	Fatal Accidents.	Deaths	Fatal Accidents.	Deaths	Fatal Accidents.	Deaths
1893 ...	4	7	4	4	8	11
1894 ...	3	3	5	5	8	8
1895 ...	2	8	18	18	20	26
1896 ...	2	2	10	11	12	13
1897 ...	3	4	8	9	11	13
1898 ...	4	7	5	5	9	12
1899 ...	1	1	7	7	8	8
1900 ...	4	5	14	16	18	21
1901 ...	1	1	19	20	20	21
1902 ...	2	11	17	18	19	29
Total	26	49	107	113	133	162
Average 10 years	26	49	10·7	11·3	13·3	16·2

This table includes not only the accidents which happened while men were being raised or lowered, but also a large number of cases in which ropes or chains broke and let trucks run down inclines and strike persons.

Owing to the increase in the number of fatalities from ropes and chains breaking during the last few years, as shown by Table 63, a return was obtained from the mine owners in the Kingdom of all breakages of winding ropes *by which men were*

raised and lowered, no matter whether the accident was attended by personal injury or not. The results are summed up in the following table:—

TABLE 64

Summary of Returns of Breakages of Winding Ropes by which men were drawn in all Mines, under the Coal and Metalliferous Mines Regulation Acts, during the year 1902.

No.	District.	Causes of Ropes breaking by which men were drawn		Number of Persons.		Number of Mines where Ropes broke in which Safety Catches were in use.	Guides.	
		In Shafts.	On Inclined Planes.	Killed.	Injured.		Number of Mines where used.	Nature of Guides.
1	East Scotland	6	2	—	—	—	4	Wood
2	West Scotland	5	2	—	—	—	5	Wood
3	Newcastle	5	—	—	—	—	2	Wood
4	Durham	1	—	—	—	—	1	Wire
5	York and Lincoln	4	—	—	1	—	1	Iron
6	Manchester and Ireland ...	4	1	—	—	2	2	Wire
7	Liverpool and North Wales ...	4	1	—	1	—	1	Wood
8	Midland	3	3	—	—	1	3	Wood
9	Stafford	7	—	2	3	—	1	Wood
10	Cardiff	3	1	1	—	1	2	Wire
11	Swansea	2	4	1	—	—	5	Wire
12	Southern	6	—	8	—	—	4	Wire
							2	Wood
	Total	50	14	12	5	4	25	Wood
		64*					20	Wire
							1	Iron

* In six of these instances it was the coupling which actually broke. Four cases of rope breaking in shafts, and five on inclined planes, during the year 1902 were reported, in addition to the 64 cases; but as the ropes were not used for winding men, the figures have not been included in this table.

The table shows that there were 64 cases of ropes breaking—50 in shafts and 14 on inclined planes. Fortunately, most of these breakages happened at times when men were not being wound, and only 12 persons are reported to have been killed.

Mr. H. H. JOHNSON: I would like to ask Mr. Epton if I am right in interpreting his remarks with regard to untwisting the rope to mean that if the rope did not go back to its proper place it is no longer fit to be used.

Mr. EPTON: That is certainly what I implied, and what I think would probably be the case. If the wires would not go back to their proper place, they would have been too highly strained to be safely used.

THE PRESIDENT: It would be a very simple solution of the whole subject to open up a rope at different points, and if the strands did not go back into place to condemn the rope.

Mr. BERRY: I have opened up both new and old ropes, and the strands have always gone back into place. I think the ropes can be opened up far enough for purposes of investigation without endangering their utility, if proper care is taken and soft clamps used.

Mr. EPTON: I might say, that although Mr. Whyte disagrees with the practice of opening up ropes, Mr. Thomas went out to a mine and carried out the experiment, and came back and told me he did not consider it hurt the rope in any way. This rope is known to be still working.

Mr. MAYHEW: With regard to this question of opening up ropes, if the rope is opened by means of two Spanish windlasses and care is taken that the bar which is put through does not touch the rope, and these Spanish windlasses are put on the rope 2 or 3 ft. apart, unless the rope is in a serious condition I will guarantee that it will go back to its original position every time. As to putting clamps on the rope, I entirely disagree with it.

Discussion adjourned.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

CONTINUED DISCUSSION.

THE PRESIDENT (Mr. A. C. WHITTOME): I should like to refer to one point which has occurred to me. At our last meeting Mr. Vaughan referred to two tables giving details of accidents, fatal and otherwise, with wire ropes in mines of Great Britain. That table, of course, is most interesting, but I hope at the end of the discussion, when the authors are replying to the discussion on their paper, that we shall have an even more detailed table given us of similar accidents in the Transvaal. The table as it stands is not of very much use, because it is no good telling you how many people get killed unless we are also told how many people are working, and in order to make a fair comparison as to whether the system of haulage at Home is better than the system of haulage here, we must not only know the number of people injured, but the number of ropes employed and the number of men working on the mines. If Mr. Vaughan can see his way to give us such a return as that, it will be of the greatest interest to us.

Mr. JOHNSON: In reading Messrs. Vaughan and Epton's paper through carefully, and being able to digest it a little better than when hearing it read, there are one or two questions I would like to put to the authors. In the first place, there is a good deal said about the bending tests of wires. I should like to know at what point the wire is said to be broken after being tested in the bending machine. We know, as a general rule, that when a wire is bent backwards and forwards it will go on bending until you bend it once too often, and it will break. But that is not always the case. Sometimes it will go on bending after a fracture takes place before the separation actually takes place. With reference to the bending test, we are given one or two different methods of doing it. For instance, in the Government Laboratory here the wires are bent over $\frac{1}{4}$ in. radius, whereas in Germany the radius is given as equal to ·19 of an inch. We also have results obtained in the Province of Anhalt, Germany, in which the radius is not given at all. Now, that gives us practically three bases, and we have got to make comparisons between them. There is a remark made in the paper to this effect (page 201): "Table 1 shows the results obtained by the writers in the Mines Department Mechanical Laboratory in tests of wires of various sizes taken from new ropes. The ultimate tensile stress varied from 82·6 tons to 159·8 tons per sq. in. The number of bendings (180 deg.) over $\frac{1}{4}$ in. radius obtained varied from 4·9 in the case of crucible steel wire of ·128 in. diameter to 50·7 in. in the case of a best plough wire of ·051 in. diameter. To compare the number of bendings in different sized wires of different grade steels is a difficult matter. The great difficulty lies in the fact that the breaking stress of the material *decreases as the number of applications increases.*" (The italics are mine.) I take it that the authors mean by that statement that the breaking stress of the material decreases in the same ratio as the number of bending applications increase. But they go on a few lines further down to say: "Regarding the test from any standpoint $\frac{\text{ultimate stress}}{\sqrt{\text{number of bends}}}$ does not give a correct basis of comparison." I certainly agree with their latter statement, but I cannot reconcile it with their former statement, if I have read it correctly. It seems to me that the breaking stress cannot possibly decrease in the same ratio as the number of bendings increase. There has been a good deal said—principally outside these meetings—of the length of the test pieces supplied to the Government Laboratory, and some people hold we do not get the best results, because the test pieces are too short. But the writers appear to have no doubt at all on the point. They say, on page 203, "they feel convinced, as previously stated, that the test of a short specimen of the whole rope, if carefully carried

out, is a fair test." Why, then, do they order the new testing machine to take pieces up to 20 ft. in length? It seems rather unnecessary to saddle the country with that big machine if they get satisfactory tests from smaller lengths. On page 208 there is a reference made to Mr. Biggart's experience. It says: "Mr. Biggart's experience went to show that the severity of bending largely determined the life of a rope, and that generally a rope lasted twice as long when only bent in one direction as against those used under alternate stresses of flexure." I am a great believer in the Whiting hoist, and I should like to know how long a rope ought to last on a Whiting hoist. It seems that we could not expect to get a very long life, and, of course, there are certain known cases where the life has been a bit short. But if Mr. Biggart is correct, we ought to find that the "over" wound rope on a hoist should last twice as long as the "under" wound; I merely ask "Do we?" Table No. 7 is a very interesting table to me. It goes into a question rarely gone into by resident engineers on the mines. That is the factor of safety of ropes when the bending stress is taken into consideration. If you will glance through that table you will notice that it gives an alteration in the factor of safety when the bending stress is included. In vertical shafts the mean factor of safety, excluding bending stress, is 15·8. In incline shafts the mean factor of safety, without bending stress, is 22·6, whereas when the bending stress is included it is reduced to 5·1 and 3·6 respectively. That seems to show a most extraordinary difference, and I can hardly believe it is entirely due to small sheaves, but should like the authors to state whether that is the cause of these low factors of safety, or whether it is due to anti-friction rollers in the shaft. There is a statement made on page 213 which I do not think could possibly have been made seriously, or else it must have been a misprint. Talking about the chemistry of the steel, they go on to say: "The fracture of metal which has been thus exposed, if moistened while still warm from the effort of breaking, froths and gives off copious bubbles for 30 or 40 seconds, and even the unbroken metal evolves gas bubbles when first immersed in water, especially if the latter be warm. The frothing power is destroyed, and the flexibility nearly, and perhaps quite completely, restored very rapidly by heating the metal slowly by simple exposure to the atmosphere at ordinary temperatures." I cannot see how by simple exposure to the atmosphere at ordinary temperatures you are going to make any impression on the metal. I think that probably wants explanation. I have to join in the chorus of other gentlemen who have regretted the reference to Mr. Comstock about American practice. I do not think that is quite true. I cannot imagine that even in America they would run their ropes until they broke, and in a valuable paper of this description I think it is only a little bit of frivolity.

Mr. VAUGHAN: Replying to the question Mr. Johnson raises, without prejudice to a fuller reply later, I can say now that in testing the bending of wires the actual fracture of the material is the deciding point. The elongation is not taken into account, and nothing really tells you that the breaking point is reached until the wire actually breaks. It is merely a question of bending the wire over a certain radius a number of times, and until the wire fractures. With reference to the latter points raised by Mr. Johnson, these refer to what are purely quotations, and I think Mr. Johnson will find they are inserted in the paper in quotation marks. We felt in writing the paper that we had to take into consideration what authors of standing had written with regard to wire ropes. The treatise of Marion Howe is an extensive work, and very valuable. Mr. Comstock's article was quoted from *Mines and Minerals*, and I think when our paper was read that a mine manager here, with, at any rate, some American experience, backed up Mr. Comstock's remarks to a certain extent, and if any manager of equal or greater experience wishes to traverse these remarks, we should be very pleased to hear them. I notice Mr. Johnson does not speak from experience in deprecating Mr. Comstock's remarks regarding American practice, and we should welcome any remarks bearing the full weight of experience of American practice. Of course, a good deal of testing work is done by Americans. We quoted from laboratory practice in California, which shows that the importance of testing work is not lost sight of. *Mines and Minerals* is a standard journal, and Mr. Comstock writes articles from week to week there which are expressive of perhaps average thought in America, and I think we did perfectly right in quoting him.

THE PRESIDENT: There is one point which Mr. Johnson brought up which I think is worthy of considerable discussion, and that is the effect the bending stresses have on the factor of safety. It is rather startling to find from the authors' tables that the factor of safety in incline shafts, excluding bending stress, on the mean of three ropes was 22·6, and, including bending stress, was only 3·6; but I think you will find that the reason the compulsory factor of safety is so comparatively high is that it is, as provided by law, calculated solely on the static load. A certain proportion of the factor is taken to make allowance for bending stresses, and another portion for the extra bending stress due to small sheaves, and another portion for the extra stress whilst turning from the vertical to the incline. I think we may take it for granted that out of a factor of seven about four would be due to the dead weight alone hanging from the rope, so that, though it looks startling to have the factor drop so low when the bending stress is included, there is nothing to be seriously alarmed about. This is one of the most important points touched upon in the paper, and I think we ought to have a little more erpression of opinion from resident engineers on the subject.

Mr. LEUPOLD: The reason why the factor of safety on incline shafts for static loads is so much greater than in vertical shafts, and also why the factor of safety is reduced after consideration of the bending stress, I think is due in great part to the fact that, for incline shafts, in order to resist the wear that takes place through the rope sliding on the ground, ropes are made of thicker wires, and generally heavier than would be necessary otherwise. This, of course, would produce a factor of safety much larger than would be the case if the mere weight was provided against. On the other hand, at incline shafts on the Rand generally, the pulleys and sheaves are small, being often only 5 or 6 ft. in diameter. Bending these thicker wires over smaller sheaves produces an extra bending stress, and hence the factor of safety is reduced in a greater degree than if thinner wires were bent over a larger sheave, as is the case with a vertical shaft. I believe that may account for the great reduction in the factor of safety for incline shafts as against vertical shafts.

Mr. WHYTE submitted a small sample of worn galvanised strand for inspection, as promised at the previous meeting.

Mr. DOCHARTY: Referring back to my remarks at the last meeting, when I brought up the question of using nickel steel in wire ropes, I have since been able to turn up some information on this subject, and I find there is nothing new at all in the drawing of high strain steel wire from nickel steel. The information I have is that it has been in the use of the American Navy for many years, and it is used in the manufacture of torpedo nets. From the steel they use, steel wire is drawn containing 30 per cent. of nickel up to 90 tons of breaking strain per sq. in., and impervious to corrosion. I should like to impress this on the rope makers in town, so that they can find out more particularly if they can get us a higher breaking strain than that. The fact that it is entirely impervious to corrosion seems to me to be an important point. There is one other point. The paragraph in the paper having reference to reheating the wire at ordinary atmospheric temperatures is impossible for me to understand, and I should be pleased to have some explanation of this particular paragraph.

THE PRESIDENT: Are you questioning the English solely, or the fact that the wire would be completely restored?

Mr. DOCHARTY: I cannot understand if you have got a heated wire how you are going to heat it again with the ordinary atmospheric temperatures.

Mr. VAUGHAN: I think it is hardly necessary for me to correct the English in this paragraph. It is simply a question of—well—a question of understanding. You know—like the celebrated sentence in which, when the punctuation was omitted, it did not read quite so well as when the punctuation was put in. The same punctuation is adhered to now as occurred in the standard work. I think I could read the sentence so as to be quite intelligible to the audience here. Evidently the adversative "but" is intended, though not actually expressed in the text.

Further discussion on this paper was postponed, and the meeting then terminated.

Dr. J. MOIR (contributed): We are very thankful to Mr. Heymann for his carefully conducted experiments on corrosion, which ought to dispel a number of superstitions which seem to be current in engineering circles. Mr. Heymann complains that the authors of this paper have not sided with

either of the controversialists on the question of CO_2 or H_2O_2 as the cause of corrosion. Well, the fact is that probably both parties are in the right, according to the conditions of the corrosion, for both H_2O_2 and CO_2 must be concerned in ordinary atmospheric corrosion, whereas in the action of mine waters H_2O_2 cannot be concerned. There is no getting away from the fact that H_2O_2 cannot exist in the presence of ferrous salts.

Mr. Heymann has also done a public service in "showing up" that Crown Deep water, which, in spite of its supposed extraordinary properties, turns out to be very like its fellows. The percentage of free sulphuric acid works out at 0·028 per cent. It is quite certain that acid of such weakness cannot attack iron with liberation of hydrogen in the cold, and that the corrosion really depends on the presence of atmospheric hydrogen, and is aided by the catalytic action of the iron salts, as explained in the authors' paper. As the authors remark, it requires strong acid to attack iron in the absence of oxygen; it is not this direct action of acids that is serious, but the catalytic action of the iron salts. Oher things being equal, one would expect more corrosion from the salts of a weak acid such as CO_2 than from the salts of a strong acid, because the former can easily dissociate into oxide and free acid, and the latter cannot do so.

With regard to the increased action at bends, I am pretty sure that the energy of the molecular strain has nothing to do with it, because it is infinitely smaller than the chemical energy of the solution of iron. I have not worked it out mathematically, but it is pretty certain that the potential energy at a bend is less than $\frac{1}{1000}$ of the chemical energy available.

The actual effect is more likely to be due to a porous condition of the metal under strain, so that it is rather a separation of crystals than a strain of molecules which takes place when the wire is bent, and this separation gives a larger surface for the acid to act on.

I thoroughly support what Mr. Heymann says about the statement of the results of analysis of mine waters. Only actual acidity to Congo-red or methyl-orange (not litmus or phenolphthalein) should be counted, and the method of determining total SO_3 and subtracting that corresponding to the iron found is quite fallacious.

Mr. Heymann's sixth experiment shows that galvanising gives a very considerable protection, even though zinc is much more easily dissolved by acids than iron. With waters containing only 0·02 per cent. of free acid like the Crown Deep water, the direct solution of the zinc in acid need not be feared.

However, if the coating gets injured, there is some doubt about the result. Theoretically it should still protect the iron, even when the latter is exposed, because the iron is the electro-negative element in this galvanic couple, whereas with copper or tin the iron is electro-positive and gets attacked if the protecting coating is injured. Probably, however, the protection of zinc is only slight after the iron is exposed, because the two metals are so close in the galvanic scale. Mr. Vaughan informs me, however, that zinc is used with great success in marine boilers in order to prevent corrosion by making the iron plates electro-negative. Since this is so, there is a very strong presumption that it will be a success as applied to wires. I am doing some experiments to test the protection of zinc, and hope to communicate the results before the end of the discussion on this paper.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. Vaughan and W. Martin Epton (*Members*).

A Correction.

Mr. Mayhew: With regard to Messrs. Vaughan and Epton's paper, a statement was made during the discussion by a member of the Council which I have since found was incorrect. Mr. J. A. Mills at a meeting held two months ago made a certain statement in which my name was mentioned personally. It was a question of galvanised versus black ropes, and Mr. Mills said:—

> "Since the War we have had occasion to put in two new ropes on 6ft. drums, 6 ft. headgear pulleys, and compound shaft. One rope was supplied by Mr. Mayhew, and the other (galvanised) was supplied by Mr. Whyte. The non-galvanised rope ran for two years and two months, and Mr. Whyte's rope ran for two years and five months. The

results were slightly in favour of the galvanised rope. In cutting the rope the galvanising in all cases was intact."

Mr. Mills promised me that he would attend this meeting and make this correction, and I would rather that he had explained the matter. The Manager of the Company informed me that both ropes were put on on the 6th February, 1904, and when Mr. Mills made his statement neither of the ropes had been taken off. The rope supplied by Mr. Whyte was taken off on the 18th June, 1905, and the rope supplied by myself was taken off on the 16th July, 1905, thus proving that the non-galvanised or "black" rope lasted about one month longer than the galvanised rope.

THE PRESIDENT (Mr. E. FARRAR): Mr. Mills is now quite a long way out of town. In regard to the statement of fact, that will be noted.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

CONTINUED DISCUSSION.

Dr. J. MOIR (contributed): In my remarks printed in the June number I promised to communicate the results of some experiments I was doing to test the value of galvanising as a protection for winding ropes against acid water. I now fulfil this promise.

Probably the specimens which I exhibit will explain the method of experimenting better than any description, and what follows is merely given for the sake of putting the method on record, and I can only hope that most of the sources of error have been eliminated. (*See Table p. 139.*—ED.)

Three wires tested in the Mines Department, Mechanical Laboratory (Nos. 124 H, K and M), were each divided into two portions, one of which was galvanised by dipping into molten zinc covered with zinc chloride. The effect of this treatment was tested as regards torsion and tension in the Mechanical Laboratory.

For the chemical test, equal lengths were prepared, and a portion ½ in. long filed flat to the centre of the wire. The ungalvanised wires were then coated with sealing wax, all except this filed portion, whereas the galvanised wires had merely the exposed ends waxed. In this way equal surfaces of iron were exposed, but in one case the rest was protected by an inert substance, whereas in the other the body of the wire was protected by zinc, forming a galvanic couple at the place where the zinc was filed away, of which the iron forms the electro-negative element. The actual areas of iron exposed were measured in each case and used in calculating the figures given in the fourth column of the table.

The results show that galvanising exercises a markedly beneficial effect, even when the iron is exposed, which is particularly striking when the acid is weak, as it is in mine waters. In the preceding tables the letter *a* denotes ungalvanised wire, and *b* galvanised wire. All experiments were done in duplicate, and at constant temperature.

It will be noticed that the advantage of galvanising increases as the acid is more dilute, and that even the weakest acid used is stronger than any mine water. Series M shows an average advantage in galvanising of 88 per cent., series H an advantage of 71 per cent., series K an advantage of 49 per cent. at first, and a much higher advantage in the second set where the wire had been already exposed to the acid.

In conclusion, may I point out that your printer has made me talk of "Atmospheric Hydrogen" as a cause of corrosion. Of course what I really wrote was "Atmospheric Oxygen."

Mr. J. R. WILLIAMS: In these tests Dr. Moir made on the wires, were the wires submerged in solution, or were the tests made in the ordinary way on the mine? If in the ordinary way, it does not matter how weak your solution is as long as you have any sulphuric acid present, as the water would be evaporating all the time and the acid would be left behind, and you would reach a point of finality where you would have 100 per cent. acid.

Mr. W. MARTIN EPTON: I asked Dr. Moir that question this morning, and I understood him to say they were immersed the whole time.

Mr. J. R. WILLIAMS: Then I am very sorry to say, with all due respect to Dr. Moir in the matter of this experiment, the results are of no weight whatever. What we want is commercial science, and I think it behoves us now to have a series of ex-

periments made of what actually happens in the shaft. In the Crown Deep it has been stated that you have ·08 per cent. of acid in the water. When that water drys off the rope the acid remains. You are at once confronted with the fact that the so-called weak acid may become a very strong concentrated acid with the repeated immersion and drying of the rope. Therefore I think it behoves you to appoint a sub-committee of your Institute to examine these experiments further from a very much more practical point than ordinary chemical analysis, which I do not wish to disparage.

THE PRESIDENT (Mr. E. FARRAR): I should like to ask Mr. Williams if he has experimentally proved the presence of this concentrated acid on wires?

Mr. WILLIAMS: No. But I think it is quite obvoius.

Mr. COWELL: Dr. Moir claims that galvanising improves the wire. Certainly the tests show a little increase in some ways. The elongation is practically doubled, but the torsion test is only about one-sixth, and this shows a marked defect in the wire. If you reduce the torsion test of the wire from 30 to 5 twists, you have certainly spoilt the wire.

Mr. EPTON: I may say that the galvanising was very roughly done, and I think the unevenness of the surface of the galvanising altered the torsion. If the galvanising had been done well and the wire re-drawn, it would have made a difference to the torsion, because if you get an uneven surface on the wire it will alter the torsion in a marked degree. In this case, if you will look at these small threads you will see that they are not round. Mr. Cowell may be right—galvanising may have an effect on the torsion, but I am not quite sure whether it is due to the alteration of the steel or to the unevenness of the zinc on the outside.

THE PRESIDENT: I should like to ask Mr. Epton whether the zinc showed any signs of leaving the wire in the process of torsion.

Mr. EPTON: It did not leave at all. It broke off. I have still specimens in the laboratory if anyone cares to come round to see them.

THE PRESIDENT: From my own experience on the subject, I am inclined to think that the tensile strength of the wire is decreased by galvanising.

Mr. EPTON: I would like to point out that we increased the area of the wire. I

Name of Wire (Mech. Lab. No.)	Strength of Sulphuric Acid in grams per 100 c.c.	Amount of dilute KMnO4 used ·1 c.c. = ·00055 gm. Fe.	Weight of Iron dissolved per square inch.	Breaking Load in lbs.	Percentage Elongation.	Number of Torsions in 8 in.
124K a_1	0·80% for 2 hours at 15° C.	0·47	·0065 gram	2,094	4·7	31
124K b_1		0·32	·0034 ,,	2,116	10·2	5
124K a_2		0·39	·0062 ,,	2,094	4·7	31
124K b_2		0·28	·0031 ,,	2,116	10·2	5
124K a_1	0·24% for 16 hours at 10° C.	4·92	·068 gram	2,094	4·7	31
124K b_1		0·42	·0045 ,,	2,116	10·2	5
124K a_2		4·16	·066 ,,	2,094	4·7	31
124K b_2		0·29	·0032 ,,	2,116	10·2	5
124H a_1	0·12% for 24 hours at 10° C.	1·27	·0148 gram	1,917	4·7	29
124H b_1		0·40	·0047 ,,	1,962	7·8	21
124H a_2		1·17	·0178 ,,	1,917	4·7	29
124H b		0·42	·0046 ,,	1,962	7·8	21
124M a_1	0·061% for 100 hours at 10 to 15° C.	2·90	·0385 gram	2,138	5·8	30
124M b_1		0·38	·0042 ,,	2,138	10·2	10
124M a_2		2·94	·0347 ,,	2,138	5·8	31
124M b_2		0·36	·0041 ,,	2,138	10·2	10

The average diameter of the wires was 0·103 in.

do not know whether that has anything to do with it.

Mr. JOHNSON: It seems to me that it has a great deal to do with it. The extra area of metal on these samples must increase the tensile strength of the wire, and at the same time it must explain the weakness in the torsional test. It is not receiving the same amount of support it would in any of the other tests, and, as Mr. Epton explained, it is practically united to the steel wire, so that when the zinc begins to fracture under the torsional test the fractures extend into the steel and weaken it. I expect it would show much better if the galvanising were done as we are accustomed to see it done in practice.

THE PRESIDENT: The increase of strength given here is a little over 1 per cent.

Mr. EPTON: It would be extremely interesting if some rope manufacturer would make some tests, or send out some wires before and after they had been galvanised to me, and I would make tests of them. I have had several galvanised wires given me, but I have none of the same wires before they were galvanised.

Mr. COWELL: With respect to Mr. Johnson's remarks about the outside of the wire being softer, that would be perfectly true in the case of any galvanised wire. I do not think it is possible to galvanise a wire and make it stronger.

Mr. W. S. THOMAS: The galvanising on the wires which Mr. Epton has shown us is only very roughly done, and it is what would be considered a very poor class of galvanising. Improper galvanising of wire has caused a prejudice to galvanised ropes in many mining communities. Such galvanising affects the strain and torsions of the wire considerably, but when properly performed galvanising makes only a slight change in strain and torsion. A wire would be lowered in strain by being passed through zinc bath, and I cannot understand why the strain of this piece should be raised unless it is on account of the thick coating of zinc on the surface. This zinc increases the sectional area, and, being such a thick coating, it probably adds something to the strain. If the increased area and the breaking strain per square inch of the zinc were known, it would be seen whether or not this is the cause of the extra breaking strain in the wire. Naturally, in galvanising such as this the torsions of the wire would be poor. By careful galvanising the wires can be made nearly as good as ungalvanised wires of the same size. A few years ago I tested some galvanised and ungalvanised wires of the same quality and size, and the galvanised wire was fully as good as the plain wire, though the strain may have been a trifle lower in the galvanised wire.

THE PRESIDENT: Were the wires you refer to from the same wire originally?

Mr. W. S. THOMAS: I cannot say. It is a long time ago, and I am speaking from memory. I can distinctly remember two lots of wire of the same size—one galvanised plough and the other ungalvanised plough. The tests were practically the same; if anything, I should say that the galvanised wire showed a better all round test.

Prof. PRISTER: In rambling through the Seymour Library I found in the January number of the "Anales des Mines" of this year the result of an inquiry with regard to the ropes used on mines, ordered by the French Government in 1901. The different reports sent in by the mine inspectors have been collected and condensed by Mine Inspector Delafond.

As the subject is mainly a mechanical one, I admit that it is mostly out of my line, but, seeing the interest taken by this Institute in the question, I could not resist the temptation to call your attention to it. I have not had the necessary time to translate all the article as I would have liked to; I only got the volume this morning for a few hours.

The inquiry was ordered, as stated, in 1901, and the following questions were put to the different inspectors:—

1. What mines possess machines for testing ropes, and what is the maximum effort which these machines are capable of exerting?
2. Have these machines ever been found too weak for the strength of the ropes? And if so, how has the test been made?
3. When ropes have been discarded, was their disuse coincident with a very great loss of strength at the socket? What was the average reduction of this strength?
4. At the deep shafts where the cables are carrying heavy loads, moving with great velocity, has it been found that the upper part of the ropes has

been more worn than at the socket? In such a case, what was the life of the rope? Specify for each case the depth of the shaft, the nature of the rope and its load at raising and at lowering.

The replies collected form the basis of this contribution.

Mines possessing Testing Machines. Strength of the Machines.—Fourteen mines possessed at that time (1901) testing machines; to-day the number may be larger. The strength of the machines employed varied from 25 to 150 tons per square centimetre. How are the ropes tested if the machines are of too low capacity? Sometimes the machines were found to be too weak for the required test, but very seldom. The ropes to be tested are either flat or round.

(A) *Flat Ropes.*—The three following methods were employed:—First method: Stretch the rope to the maximum strength of the machine, stop the machine, cut the two external strands, following a course at right angles to the axis of the rope; re-start the machine until the remaining strands are breaking off. The resistance of the single strand is calculated by assuming that it is equal for all, and it is further assumed that the resistance of the rope is proportional to the number of strands. This method is used by the rope manufacturers of Auby. In the opinion of Inspector Kuss, the method gives exact results.

Second method: Cut the rope longitudinally and test both halves. It is assumed that the strength of the rope is equal to the sum of the strength of the halves. From tests made in 1901 at the mines of Blanzy upon four wire ropes of eight strands each of a breaking strength of about 60 to 86 tons, it was found that the difference between the actual strength of the ropes and that obtained by the above method was not more than 1 per cent. For greater safety a reduction of 5 per cent. can be made on the sum of the strengths of the halves.

Third method: Each strand is tested separately. At Lievin, it is assumed as a result of numerous tests made upon fibrous ropes that the total resistance is obtained by adding together the strength of the single strands and multiplying the total so obtained by a factor 1·15 to 1·30, while the smaller factor is used for the stronger ropes.

(B) *Round Ropes.*—Evidently in this case the *modus operandi* must be different from that used for the flat ropes; the test of the single strand could not be employed with any confidence, because the helicaidal twisting will remain after the rope has been unrove. Therefore wire by wire must be tested. Two methods are generally used—that of the Company of Blanzy and the so-called Westphalian method. The method used at Blanzy is as follows:—

The steel ropes employed at the shaft Maugrant have seven strands of 19 wires of No. 17; their strength when new was about 115 to 120 tons. Every wire is tested for tension as well as for bending. The bending tests are only of secondary importance, as they do not disqualify. The figures obtained from the single strength tests are totalled and the sum is reduced by 33 per cent. This factor was ascertained from 26 experiments made in 1900 and 1901 with different ropes varying from 144 wires of No. 16 to ropes with 152 wires of No. 10. In all cases it was found that the laying up reduced the strength so that the total was less than the sum of the strengths of the single wires.

The Westphalian Method.—By this method the breaking strength of the wires as well as their flexibility is considered. The breaking strength of the single wires is totalled, those wires whose resistance is less than 20 per cent. of the average strength are eliminated, as well as those which do not stand a certain number of flexions. This last number varies with the diameter of the wire, increasing as the diameter is smaller. At Blanzy they declare that this formula gives wrong results. By applying it to the 26 ropes tested as above they would all have been disqualified, when, on the contrary, the direct tests had given satisfactory results.

Reasons for Discarding the Ropes.—Has a diminution of strength been observed at the socket? Value of this reduction. The information obtained from the different mining engineers can be summed up as follows:—

In the North Departments all the ropes used are fibrous, and the same can be said for the Pas-de-Calais. In the Department Saone-et-Loire we have a report upon 19 wire ropes used at the mines of Blanzy for raising men. Two have been discarded owing to severe injuries sustained during working. The other 17 were discarded for wear and tear, which was evident from the

many broken wires. The reduction of strength at the socket of 14 ropes was:—

0 to 5	per cent. for	7 ropes.
5 to 10	,,	4 ,,
10 to 20	,,	3 ,,

At Blanzy the ropes were also mostly discarded for broken wires, the reverse of what had been observed in the case of the fibrous ropes.

Mining Inspector Nentien expressed the opinion that the tests for the wire ropes do not furnish any exact diagnosis of these conditions, and thinks that this will be more and more the case, as the use of hard steel ropes of great strength becomes general. He says that regular and frequent inspection of the ropes is necessary for the safety of the men. Engineer Glaser is also of the opinion that for wire ropes the breaking tests are of less value than for fibre ropes. Wire ropes can give satisfactory breaking tests, but, nevertheless, the wires have lost their ductility, and will break at the smallest bending or shock; therefore it is the breaking of the wires that decides the discarding of the ropes.

In deep shafts where the ropes are carrying heavy skips, moving with great velocity, has a greater wear and tear been observed at the upper part of the cables than at the socket?

All engineers having declared that, independently of the depth of the shafts, the upper part of the rope was always more used than the lower part, it remains only to be seen whether this wearing at the beginning of the lift (jerk) is becoming more accentuated in deep shafts. Other figures and observations made upon fibre ropes, not having a special interest for the Rand, may be passed over.

Mr. Delafond says that we have not enough data for solving definitely the problem, and must therefore await further information from tests which he is suggesting. These tests should be carried out with the discarded ropes.

INFORMATION GAINED FROM THE INQUIRY.

The information obtained from the inquiry is rather scanty, but nevertheless some interesting points have resulted.

(A) *Wearing of the Ropes.*—The fibre ropes wear unequally, the wear manifests itself principally at the socket and at the enlevage, but the greatest wearing is mostly observed at the latter point. Under equal conditions it is probable that the wear or loss of strength is greater at the enlevage; the deeper the shaft, the heavier the load is. For the wire ropes the same could be said, but the loss of strength is less at the socket. The principal cause for discarding the ropes is the rupture of wires at the enlevage. The weakness at this point is more important than has generally been admitted. One of the principal causes of this weakness, which has never been remarked upon before, is mentioned by Engineer Glaser. The shock at the upper part of the rope is produced by the slowing down, more or less briskly at the end of the run, by the *vis viva* of the cable and the descending skip. It must be remembered that in past times the quantity of mineral extracted was less, the shafts not so deep, the velocity of the skips not so high, and consequently less shocks were produced by the varying velocities. To-day, however, there exists a new cause for weakening the ropes which did not appear and attract attention in past years. The weakening at the socket, to which more attention was given in the past, is to-day of secondary importance in consequence of the precautions taken by the more frequent changing of the lower part of the rope, by better guides, which are reducing the shocks at the lower part of the ropes, and by better springs used for the suspension of the skips.

(B) *Tests to be made on the Ropes.*—These tests can only be made at the lower part of the ropes at the time of re-capping. By themselves they do not furnish sufficient information as to the general conditions of the rope, but nevertheless they should be regularly made, as they furnish very useful information. It is certain, at least for the fibre ropes, that at the moment the tests made at the socket are not satisfactory, the enlevage is certainly in bad condition, and the rope must be changed immediately. But for elucidating a series of very important questions, which to-day are still unexplained, it appears to be very useful to submit the discarded ropes to a series of tests. In this way it would be possible to discover the laws regulating the relation between the strength of the ropes at the socket and at the enlevage, and to better obtain information as to the time when the ropes should be discarded. All engineers who have studied this question arrived at the same conclusion.

(C) *How to make these Tests.*—Often very serious difficulties are encountered in

carrying out these tests. The loads are becoming greater and greater as the depths of the shafts are increasing; the ropes must become more and more resistant, and the testing machines are found insufficient. The following methods have been used for solving the problem:—

For Flat Ropes.—Every single strand can be tested separately and the results summed up, as is done at Lievin. This total is then multiplied by a factor which is found by experience.

Another method, which seems to be preferable: a part of the rope is tested—for instance, the half. It is found that the strength of the whole rope is proportional to the number of the strands, but nevertheless it is advisable to reduce the figures obtained by 5 per cent.

Round Wire Ropes.—For these ropes the problem is more difficult. In some mines the Westphalian method is employed, but, as we said before, this method is subject to very serious criticism. The differences obtained at Blanzy between the calculated strength following the Westphalian method and the direct tests are very great indeed. Some may ask whether the tests made at Blanzy have been carried out in the same manner as in Westphalia, or whether the steel used at the different mines is not of a different quality. In France, at least, the formula should be employed with all precautions. The other method, proposed by Blanzy, is as already described—to test separately every wire and to admit for the total strength of the rope two-thirds of the sum obtained by adding all the single strengths together. This method is certainly very simple and useful, but before admitting it generally it should be confirmed by numerous experiments made at other mines. The two described methods are empirical and give only approximate results, which often vary from each other. Consequently the direct method should be used as much as possible. But here we meet difficulties of another character. First, machines able to give an effort of 150 to 200 tons are expensive; secondly, because the rope may break at the point of attachment. At the Conservatoire des Arts et Metiers in Paris a 300-ton machine has been erected and has given very satisfactory results. Delafond gives the details as to how these tests should be carried out. He advises the mine owners to send their ropes to the aforementioned laboratory where they could be tested. In conclusion, Delafond says: "Under the actual manufacturing conditions, the ropes are wearing principally at the point of the lift-up (enlevage) the weakness manifesting itself also at the socket (patte) but less accentuated. These differences in the wearing are more pronounced in wire ropes than in fibre ones. It is the lifting point that must be specially controlled and which must be, if possible, strengthened more than is done to-day. The tests made at the socket, when the rope is re-capped, are certainly useful; but it is of the highest importance that other parts of the rope should be tested, when it is discarded, in order to discover the relation existing between the weakening at the socket and the lifting point for every shaft. As the mine-owners do not possess the necessary testing machines, tests should be carried out in a central institute."

SOME PRACTICAL SUGGESTIONS ARISING FROM MESSRS. VAUGHAN AND EPTON'S PAPER, ENTITLED: WIRE ROPES USED FOR WINDING.

By G. A. and H. S. DENNY.

The valuable and highly instructive contribution on wire ropes by Messrs. Vaughan and Epton calls for general appreciation, but specially so perhaps on two points:—

(*a*) Actual value of information.

(*b*) Clear and lucid presentation.

So much do these points impress themselves on the writers that they venture to put forward a suggestion to the two gentlemen named that their investigations be pursued a little further in a direction which the writers will endeavour to indicate in these notes.

At the outset it will be admitted that from the academic point of view the contribution under notice fills a want, and the theoretical student will benefit largely by keeping it as a reference work for the matters with which it deals.

From the practical man's point of view, however, it seems to the writers that there is an absence of constructive suggestion, which would seem well within the scope of the contribution, but which has not been supplied.

On a mine, the care of and responsibility for the winding ropes lies in the hands of the resident engineer and the mine rigger. It is not to be expected that the resident engineer can always be an expert on rope construction, or the relative effects of kinetic shocks and nascent hydrogen. He knows what Lang's Lay is, and he can figure what his load is, and he knows something of the difference between crucible and plough steel; and from these data he can say roughly what size and class of rope he requires for a given duty. But the more scientific details with which Messrs. Vaughan and Epton deal, he takes interest in only in the abstract, and the multifarious duties which fall to his daily lot must almost invariably prevent his going further into the matter. Whilst therefore the manufacturer, the student, and the framers of Government regulations may gain much from the paper under notice, it is doubtful whether it goes far enough to assist the engineer in the practical handling and running of his ropes.

On page 204 of the April *Journal*, Messrs. Vaughan and Epton detail a number of causes of rope deterioration, but their treatment of the remedies to these evils is certainly incomplete.

The writers suggest, therefore, that the thorough knowledge of the disabilities attendant on wire rope handling, so clearly evidenced by Messrs. Vaughan and Epton peculiarly fits them to complete the statement already submitted by a further contribution, dealing with the proper precautions to be observed, from the hoist drums, to the furthest point from which hauling is to be carried out.

The following set of questions might be taken as a basis on which to work, and although these are not intended in any way as covering all the points in this consideration, they will serve to make the meaning of the writers quite clear.

I. Relation of drum diameter to rope diameter.

II. Method of attachment of ropes to drums.

IIA. The crushing effect on the underlaps, on a drum which has numerous layers of rope wound upon it.

III. Best angle of inclination from drum to headgear sheave.

IV. General disquisition on sheaves and guide rollers, including weight, diameter and design. The evils of too much weight in the sheaves, and the effect on the rope construction when it, by reason of the weight of the sheave, virtually acts as a brake. The necessity for careful lubrication and free movement, renewals of wearing parts of rollers, etc.

V. Method of attachment of rope to skip or cage. Various methods in use and suggested practice.

VI. Rope dressing prescription from which dressing can be made on the mine. Different dressings for varying conditions, frequency and best method of dressing, automatic appliances for constant dressing, evils of neglect of dressing and evidences of neglect of dressing.

VII. Rope deterioration—evidence of and danger signs.

VIII. General printed regulations for the use of mine engineers.

IX. Sampling water for acid tests.

X. Twisting and kinking of ropes—how to be avoided or eliminated.

XI. General and sundry information.

In the opinion of the writers, deterioration of ropes is more generally due to negligent handling than to faulty construction.

The diversity of opinion in regard to all of the above points, so noticeable as between mine and mine, is in itself evidence that there is room for a proper investigation of all these matters.

As before stated, Messrs. Vaughan and Epton have so great an interest in the subject, and such conspicuous ability in dealing with it, and, moreover, their position in regard to the industry will so facilitate the collation of the data necessary to deal with the above points, that the writers believe they are particularly fitted to deal with them.

The writers are prepared to submit all information regarding the practice on the mines with which they are connected which bears on the question of wire ropes, and they venture to think that there would be no

difficulty in obtaining the same information from all the other mines on the Witwatersrand.

The writers desire again to express their great appreciation of the value of Messrs. Vaughan and Epton's communication, and trust that they will add further to the indebtedness of the Rand engineers by making their views known in detail upon the lines of the practical suggestions which the writers have made.

Mr. WAGER BRADFORD: There is one thing I should like to say with reference to Mr. Williams' remarks on the concentration of acid water on the rope. In deep level mines, most of the acid water occurs below the vertical portion of the shaft. In the vertical, although there is a great deal of drip, the water is comparatively pure, as it does not come in contact with oxidising mineral formations. It is the water draining through the stopes to the inclines which carries the acid, and for this reason, if concentration of the acid does take place on the surface of the rope, the portion running in the incline should show much more corrosion than the upper part. In practice this actually occurs, and that part of the rope most frequently immersed, viz., the last 200 to 300 feet, is usually the first to go, while the portion running in the comparatively dry incline, where the water is flowing on the shaft bottom, is less injuriously affected.

Mr. A. W. K. PIERCE: Harking back to the samples furnished by Dr. Moir in connection with his test, the result of the imperfect galvanising would appear when putting the wire through the torsional machine, and it would start to go where the diameter was small. The twist in the wire would not be uniform throughout the length between the jaws, so that the twist per inch of the actual section which broke would be quite high.

Mr. W. MARTIN EPTON: I think that is quite true, and is most probably the cause of it.

Mr. A. W. K. PIERCE: And, therefore, it would not apply to a piece of rope which was galvanised by a commercial man.

Further discussion was then adjourned.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

CONTINUED DISCUSSION.

Mr. F. A. BULLIVANT: I am extremely obliged to you for the opportunity you have given me of making a few remarks on the extremely able paper under discussion, and I cannot but compliment the writers on the very interesting statistics with which they have filled the paper. Unfortunately I have only just arrived in this country, and have not had time to go thoroughly through the paper, and the discussion thereon. However, I would like, with your permission, to make a few remarks.

I note that the paper deals only with the local working conditions that exist on either the gold or the coal mines here, but so thoroughly have the writers of the paper worked up their subject, that, so far as I can see, they practically apply to mining all over the world. I quite agree that the duration of the life of a winding rope depends to a great extent on the suitability of the work it is called upon to perform, and great importance, in my opinion, should be attached to this. The writers of the paper state that they do not attach much importance to the loss of toughness and ductility which is obtained in working the extremely high tensile ropes at present so much in use in this country; and here I would rather like to differ with them, as my experience of the last 25 years of the testing of steel wire, of every grade, shows me that there is at times a very considerable brittleness exhibited in the high tensile strained wire, and therefore a considerable loss of ductility. I have myself tested tons and tons of this quality of wire, and when drawn to strains of 130 English tons on the square inch and above, there is no doubt that you can take a parcel of this wire which gives at the ends of the bundle most excellent results for ductility, but on cutting these bundles in the middle and testing, you will get a very different result.

I have brought over with me, for the purpose of the Government Commission which is about to sit, samples of wire bearing out the remarks I have made, but unfortunately

the case in which they are contained has not yet arrived here, otherwise I should have exhibited them to the Institute. In fact, I may say that my experience is the same as that quoted by Mr. Kenneth Austin, that there is too little consideration given here to the torsional test and too much to the tensile strength; some of the finest samples of the working of winding ropes having been obtained from wire drawn to a very much less strain on the square inch than that in general use here. Neither am I inclined to agree with Hrabak when he states that the torsion test appears to him to be of less importance than the bending test. I have often found in the testing of wire, that where good bending test is exhibited, it is not always accompanied by a good torsional test, but, on the other hand, good torsional tests are always accompanied by good bending test. And here I may remark incidentally, that it is a very good plan in taking a torsinal test to put a chalk line along the wire, and if this is done, the spiral lines produced by the rotations are clearly shown along the length of wire.

I have read with very great interest the remarks quoted from Hrabak, with reference to what he calls the stiff strand, and I quite agree with the writers of the paper that the core wire in the strand is very much preferable to the hemp core, provided always that the core wire is of a sufficiently soft nature.

There is no doubt that the fact of a hemp core being in the strands of a winding rope, is apt to displace the wires after the rope has been in use for some time, more especially if riding turns are allowed on the drum, which I understand is almost universal here.

I may state that I am in accord with the writers of the paper when they state that they favour a standard specification for winding ropes, and that the tensile strength and ductility should be stated in a more exact manner.

With reference to the remarks extracted from Messrs. John A. Roebling, Sons and Co.'s catalogue, my experience in testing of rope coincides with the writers of the paper, and I have never found the nicking or crushing effect of one wire upon the other in a Lang lay rope. This, however, I have noticed when the strands have been made of a compound construction and laid with the inside wires in an opposite direction to the outside wires, and I believe that I have in the case of samples, to which I referred before, samples showing these nicks produced when the rope has been under strain in the testing machine. I have examined with very great interest the tests made by the writers of the paper at their laboratory, and I congratulate them on the results which they have obtained. In my opinion, the methods adopted by them are fully in accord with modern practice, and I also agree with them that the tests conducted on short lengths of rope are equal to those conducted on longer lengths.

The tests of rope which my Company have to make are, of course, extremely numerous, and the method used by the writers of the paper would take up a great deal too much time in busy works such as our own, where we are practically testing all day, and frequently all night. We therefore use a form of grip for the rope, of which I herewith hand you a diagram for your inspection. You will see that this diagram is marked as showing the method by which the Admiralty insist that ropes should be tested, this method having been adopted from us.

But I say at once that the methods adopted by the writers of the paper generally give a better result in testing than those obtained in grips, as it very often happens that the ropes will break under the compression of the grips; but so long as the ropes thus tested give the strain required, it is sufficient for our purpose, and no doubt in the case of rope having broken in the grips, had it been tested in the manner adopted by Messrs. Vaughan and Epton, the results would have been a little more favourable.

I read with the very greatest interest that the Mines Department Mechanical Laboratory are putting down a 200 English tons Buckton horizontal testing machine, and should they, when the machine is erected, require to test it up to its fullest capacity, my Company will have the very greatest pleasure in sending them a rope or ropes for this purpose, if they will kindly state exactly what breaking strain they would like. Whether it should give 198 or 199 tons, I can promise them a rope with the exact strain that they require.

Circumstances that increase the external wear of ropes.—I have read the remarks made by the writers of the paper and they practically agree with mine, but I would like to add to the list which they give on p. 205 of the Journal, first column, " decrease

in the sectional area of the wire in the rope as well as the breaking of the wires," viz., in addition to those already mentioned by the writers, I suggest that great care should be taken in the capping of the ropes to see that sufficient length of service or binder is securely put on the end of the rope, otherwise, with the high tensile strained wires which are in use here, it will be found that, unless great care is taken, some of these wires will spring. And if this is not noticed by the rigger, these slackened wires, directly the rope comes into use, will circulate for a considerable distance up the rope, thus causing what are known as " high strands " or " proud places," and these, when the rope is worked, will take more friction than the other wires in the rope. Also the same care should be used in uncoiling the rope, as turns thrown out of a rope will produce exactly the same effect as I have mentioned in connection with capping. Of course, this is obviated provided the rope is taken direct from the reel, and the remarks only apply where the rope is taken from the coil.

I do not propose to make any remarks with reference to the chemical part of the paper, in fact, the subject is quite beyond me, but I would like to make some remarks with reference to galvanising, of which I have had a long experience, and I quite agree with the remarks made by Mr. J. Bucknall Smith, that the elongational efficiency of steel wire, and also its ductility, is diminished by the process of galvanising, and I am glad to see that I. am in accord with Mr. Mayhew on this subject, and this applies especially to the high tensile strain wire in use here.

Regarding the deterioration due to fatigue or change of state of the steel, I quite agree that the frequent application of a load, well below that producing stress up to the elastic limit, eventually produces this in steel, and that the fatigue shows itself by the steel becoming brittle.

With reference to the remark on p. 214, col. 1 of the *Journal*, that Mr. Bucknell Smith quotes a case in which a rope sample with all its component wires cut some 18 ins. in advance of one another lost very little in strength, this, in my opinion, would be quite correct. In fact, it is well known to me that a rope cut in the manner described, only a lay and a half away, will give the same result.

I quite agree with the writers of the paper that no serious consideration need be given to the fact of latteral support in the wires of a worn rope, but that the strength of the rope should be assessed from the amount of steel remaining at the most worn places in each wire. As a manufacturer, I quite agree with Mr. J. R. Cowell's remarks, that my experience has shown me that, unfortunately, there are agents only too anxious to make business, who will take an order and supply a rope without any practical knowledge of what they are doing, and trust to providence that they are all right, and I quite agree with him that the success of a rope manufacturer can only be due in the end to care taken to supply suitable rope for the work required, and here I may mention incidentally that it is the custom of my Company never to send out a rope unless it has been thoroughly tested before, and the actual breaking strain taken. By thoroughly testing I mean that every wire put into the rope has already been tested for tensile strain and ductility.

The President (Mr. E. Farrar): With reference to Mr. Bullivant's preference to low grade wire, we all have that preference, but unfortunately in deep level shafts we cannot adhere to it. We must have a high tensile strength, or we would not get a rope to carry its own weight down the shaft.

Mr. W. S. Thomas: The authors of the paper before us, on " Wire Ropes used for Winding: Their Strength and some Causes of its Reduction," hold the peculiar position of being able to examine ropes of almost all makers, good, bad and indifferent, and in all stages of wear and decomposition. That they have made good use of their opportunities may be seen from the very able and valuable paper which they have presented to us on the subject. The paper is of great value to rope makers as well as to those under whose supervision wire ropes are working. The thanks, therefore, of all who handle ropes are due to the authors for the pains they have taken in compiling this vast fund of information and in presenting it to the public.

No satisfactory method of comparing the duty of ropes in various shafts has yet been discovered and the formula $\frac{\text{mileage run}}{\text{factor of safety}}$ certainly will not be found to answer in any case. This formula does not take into account the factors peculiar to each shaft, such

as the class of engine used, size of drums and pulleys, the lead to the engine, whether the shaft be vertical, inclined or compound, the class of rollers used in the turn and on the road, corrosive action of the water, and last, but not least, the engine driver. In certain cases it is necessary to put a very flexible rope in a shaft where a rope of larger wires would be better, were it not for one or two local conditions. The flexible rope is more costly to start with and will not last as long.

For these reasons, it seems impossible to deduce any formula by which we may compare the duty of ropes working in different shafts. We have cases of ropes working for years and others doing less work, lasting a shorter time, the quality of the steel being similar in each case.

The writer cannot agree with the authors of this paper when they state that higher tensile strains are always obtained at the sacrifice of ductility, nor, with Mr. Kenneth Austin's views on the same subject. Some time ago the writer supplied wire ·082 in. diameter drawn to 147·2 tons per square inch with an average torsion of 35½ in a length of 8 in. The highest torsion was 45 and the lowest was 28. The bends were the same throughout, i.e., 10. The average number of torsions in another lot of wire of the same size, in a very good quality of steel drawn to 134·4 tons per square inch was 41·6, thus proving that ductibility is *not* greatly sacrificed in obtaining higher tensile strains.

The average torsion of the 147·2 ton wire mentioned above is higher than that of the wires quoted from Hhabak, and also higher than the " standard " which Mr. Austin gives. In the latter case, the torsions for ·080 in. diameter wire are 32 when drawn to 120 tons (2,240 lbs.) per sq. in. The wire size estimated on is smaller, and the number of torsions should therefore be above the ·082 in. diameter wire. This ' standard " torsion given by Mr. Austin seems lower than should be the case with a good quality of wire.

Referring to Table 1, Tests Nos. 113 and 114 show the torsions for two pieces of ·073 in. diameter wire, drawn to 135 to 136 tons, to be 38·7 and 34 respectively. As this lot of wire drawn to 147·2 is larger in diameter, the ductibility is greater at this strain than at the lower strain. In this connection it might be pointed out that the torsions of the ·099 in. diameter wire drawn to 159·8 tons per sq.in. are very low indeed. (Test No. 83.)

Mr. Bullivant's statement that the middle portions of hanks of wire which have been drawn to very high strains are not as good as the ends, does not appear to the writer to be reasonable, nor does this statement appear to be borne out by fact in the case of this particular lot of wire. Both ' ends of every hank used in making this rope were tested and each end gave practically the same test. The middle of the hanks could not, of course, be tested, but it is hardly possible that any wire could be good at the start, poor in the middle and good again at the other end. Were a hank to become poor in the middle it would in all probability get worse towards the end which passes through the die last. The rope which was manufactured from this wire was placed on one of the Whiting hoists here on the Rand and performed about 3½ million foot-tons more work than the rope preceding it, which rope was probably of steel drawn to 105/110 tons per square inch. Had the middle of the hanks of wire been defective, as Mr. Bullivant has found in testing pieces of this strain, the fact would have shown in the working. As a matter of fact, the rope was removed on account of corrosion at each end, and the middle portion, which was never left hanging in the shaft, proved to be the best portion of the rope when removed.

The authors state that they are in favour of a standard specification for hauling ropes. In the writer's opinion, the only addition advisable to the present form of certificate is to make a standard percentage of deduction from the aggregate strength of the wires for each of the various constructions. At the present time certificates contain the size of all wires, and the strain per sq. in. to which the steel is drawn, so that it is really a simple matter to find whether the breaking strain given is the aggregate strength of the wires or less, and, if less, by what percentage? If the Mines Department should make a standard percentage of deduction from the aggregate strength of the wires for every construction, this would make the rope breaking strains uniform, which is all that is required.

A rope which is not made of a good quality of steel will clearly evidence the fact after a few months' work, so that any regulation regarding the number of bends or torsions

that each wire should stand seems unnecessary. The writer has personally tested two qualities of steel in which the mechanical tests showed them to differ very little, but in subsequent work the one proved over 25 per cent. better than the other.

In England, Lloyds require a certain test for all hawsers and flexible ropes used in shipping work. This test is as follows:— The wire must be drawn to a strain of 90 tons (2,240 lbs.) per sq. in., and must be capable of being twisted eight times around its own diameter and back again without breaking. Amongst good rope makers, any wire which will not stand far *higher* tests than the above is rejected, even for hawsers, which are not required to work around drums or pulleys.

If a law be passed here requiring wires to have certain torsions, the standard will, necessarily, be made low, and in all probability this will lead to a false security, from the fact that a rope made of wire which will pass the Government requirements might be considered as safe and as good as another of a far better quality than that required by the Government regulations.

The percentages to be deducted for loss in spinning, as shown in the authors' Table II, vary considerably, but from a careful consideration of the table it is safe to assume that the results obtained from good makers' ropes show fairly regular tests. From a series of tests made for the writer on Messrs. David Kirkaldy and Sons' machine, fair allowances to be deducted for loss in spinning seem to be as follows: 6/6 and 1 construction with hard core wire in strands, using 42 times the average breaking strain of each wire, deduct 7½ per cent. to 8½ per cent.; 6/6 and 1 construction with soft core wire in strands, using 36 times the average breaking strain of the outside wires, deduct 3½ per cent. to 4½ per cent.; compound construction 6/7 over 6, 6/8 over 7, 6/9 over 7 and 6/10 over 7, using all wires. deduct 8½ per cent. to 10 per cent.; 6/19 construction, using 114 times the average breaking strain of each wire, deduct 14 per cent. to 15 per cent. In all certificates made out by the writer, the foregoing are the deductions made in calculating the actual breaking strain from the aggregate strength of the wires. The series of tests which determined these percentages were very regular, and there was not the variation shown by the authors in Table II. It is probable that many of these high percentages noted in Table II. were due to either a faulty construction in the ropes or to an uneven strain on the wires in testing.

The method used by the authors of gripping the ropes in testing appears to the writer to be as good as any other method, if the samples are carefully prepared. In this connection, the writer wishes to say that in almost every case when samples have been submitted by him to be tested the results obtained by the mechanical laboratory have been very nearly the same as those obtained on Messrs. Kirkaldy and Son's or the Sheffield Testing Works' machines. On these machines a length of 100 in. is usually tested, but this does not appear to give an appreciably high breaking strain in the ropes. For this reason we must conclude that the results obtained in Tests 145, 157 and 35 in Table II, which show 12 per cent. to 14·4 per cent. loss for 6/6 and 1 ropes, and in Test 58, which shows 17·4 per cent. loss for a compound construction 6/10 out, must be due to a faulty construction in the ropes.

In Table V the loss in breaking strain per square inch of the steel is quite remarkable. As the torsions and bends are both low after immersion in the acid solution. this would seem to show that the acid affects the structure of the steel. Part of the loss in the breaking strain per square inch is due to the fact that the surface or skin of the wire is removed and part of the remaining wire is attacked though not entirely dissolved, and remains on the wire as a black coating. It would be interesting to know whether the structure at the centre of the wire is changed. This could only be seen by examining the break with a glass. The low bending and torsional tests would indicate that the structure of the steel is changed completely by the immersion in the acid solution.

Some time ago a peculiar failure of a rope was noted by the writer and a full explanation has not yet been found. The peculiarity was this: The structure of the steel changed with but very slight signs of pitting on the surface of the wire. The part affected was usually about half the diameter of the wire and occurred at the point where the wires turn into the rope—a place where there is no wear. The half of the wire which was affected broke at right angles to the axis of the wire, and

the other half broke as an ordinary good wire should do. Under the glass the bad half appeared as though oil had been forced into it under pressure. In every case the wires broke in the full section and not where the section was reduced by wear. Had the wires broken in the worn portion, the explanation would probably have lain in the fact that the wire was drawn too high in strain for the quality of steel, and the constant friction had crystallised the steel.

This peculiar change in the steel was exhibited in two ropes at the lower end. One piece taken 250 ft. from the lower end of the first rope gave a good test in the laboratory—a piece from the same position in the second rope did not. Upon cutting the second rope back about 600 ft., and testing the portion there, a good test was obtained—the wires all breaking well. The peculiarity of the break had all disappeared which proved that some outside influence had affected the rope, and it was probably something in the mine water.

The authors, quoting from Henry Marion Howe, say that: "Steel exposed to nascent hydrogen will suffer many of the effects noted in this wire or, at least, become brittle without showing the effects of corrosion, *i.e.*, pitting or being eaten entirely away." In fact, it is stated that this effect is inversely as the corrosion in the case of the steel being used as an electrode. The writer does not wish to explain the failure of the above-mentioned rope as due to the action of nascent hydrogen absorbed by the steel when subjected to the influence of the mine water on these fields, but suggests this as a possible explanation of a peculiar failure which, so far as his experience goes, is entirely new.

Professor Arnold's lecture, which many of us had the pleasure of hearing a few weeks ago, was extremely interesting, if only in showing us how little is really known of the metallurgy of steel, and what possibilities there are in this field for close observation. In the rope referred to, we have a new condition, and it is possible that more light on the effects of nascent hydrogen may explain away the present difficulty.

In Tables VI and VII the authors have given us the ratio $\frac{D}{\delta}$ and factors of safety worked according to Hrabak's formula, when these ratios are taken into account. At first glance, these low factors sound somewhat startling, but in the writer's opinion there need be no fear, for the simple reason that when this ratio is too small the effect of wear will be exhibited after a short period of use by the cracking wires throughout the rope. A good rope would never break, though being bent around small drums and pulleys before showing many broken wires, sufficient to cause its removal. There has been only one case, so far as the writer knows, where the fact that the ratio $\frac{D}{\delta}$ was small was the attributed cause of a rope breaking on these fields, and there is little doubt in this special instance that the break was due to some other cause, as the same construction is being used on this shaft at present and seems to answer best for the conditions.

Too much stress is laid upon this ratio so far as safety is concerned. The interest of the mines with the small ratio $\frac{D}{\delta}$ is really concerned in the *cost* of the ropes, not in their safety—*provided* of course that we have always the proper supervision of ropes required by the regulations. The extract given from the experiments performed by Mr. Biggart seems to imply that Mr. Biggart takes this view of the question, as nothing is said about the danger of using ropes over small sheaves and drums.

The general practice in America and, as Mr. Vaughan states, the law in Austria, is to have this ratio 1 : 1,200. With the sizes of the drums and pulleys used on the Rand, to have the diameter of the largest wire 1,200 times less than the diameter of drums and pulleys would mean 6/19 ropes in nearly every case. These 6/19 ropes would be more costly to start with and would not last as long, so that such a ratio here would mean thousands of pounds every year to the mines and would not ensure greater safety. As a matter of fact, the 6/19 rope, if generally adopted, would prove less safe in many cases on account of deterioration through corrosion—the smaller wires being more reduced by the same action, on account of the greater surface exposed.

Another reason to be advanced against having this ratio large is, that in some cases, where large and heavy sheaves are put in, the inertia causes these sheaves to revolve after the rope has come to rest, thus causing great wear on the wires during the retardation period.

Table VII is very interesting, and confirms the conclusions arrived at by experiments made by our Home works, *i.e.* that it is impossible to formulate any rule by which the breaking strain of worn ropes may be always found. If there is but wear alone and corrosion has not affected the wires at all, a fair rule would be to calculate the strength of the rope from the section remaining, without deducting anything for the loss due to spinning. This should in every case give a lower breaking strain than the actual, and so we should be on the safe side in following this rule. The writer does not believe that in cases where excessive corrosion is present *any* rule for finding the breaking strain of a worn rope will hold.

Unfortunately it is very difficult to measure accurately the amount of section remaining, especially before the rope has been taken apart. One is liable to make a considerable error in calculating this even when calculating on the section by measuring each individual wire. In Table VII, the fact that so many of the ropes examined differ to such a great degree in the breaking strain per square inch of the steel before being worn, and after, would indicate that either something other than wear has affected these ropes or that the worn section is not correct.

Ordinary wear might change the strain of steel by about 5 tons per square inch either higher or lower, but it is not likely to change it more. There are 12 tests in which the strain per square inch is altered from the original more than 5 tons per square inch. If the worn section as calculated be correct, it shows us that some sort of corrosive actions affect a considerable number of ropes.

The writer cannot agree with the authors when they state that apparently there is no gripping of the wires in a worn rope, and this can be proved by the majority of results in their Table VII.

All the constructions shown will break at 8½ per cent. to 10 per cent. less than the aggregate strength of their wires when new, so that there is a point to which the ropes must be worn before a zero is reached. If this zero point be assumed to be 90 per cent. of the original section, many of the tests which show a minus percentage in the last column would be on the plus percentage side when this is taken into consideration. The majority of these minus percentages are less than 10 per cent., which is the factor to be deducted when the rope is new, so that this proves a certain amount of gripping when the wires of a rope are worn to their positions.

In an old rope, corrosion, when affecting the inside of the wires in either the strand or next to the hemp core of the rope, would tend to prevent the wires from gripping, and in some cases would make the rope break lower than the aggregate of the individual wires. As pointed out in Table II, faulty construction will make this percentage to be deducted for spinning much higher than 10 per cent. When all of these facts are taken into consideration, we shall have to come to the conclusion that there is a gripping of the wires in worn ropes, and in many cases this is very considerable.

It will be noted in Tests 49, 78 and 79 that the minus percentage is high. This is probably due to the construction of the ropes tested. In the 6/9 over 3 construction (Tests 78 and 79) to form a round core in the strands, the inside wires must be spun in the opposite direction to the outside. When a rope of this construction is worked over pulleys, these wires having different directions cut each other. In several instances in this country where the 6/9 and 3 construction has been used the inside wires of the core have broken, and in the writer's opinion, this often happens without being evident when examination is made of the outside of the rope.

The △ core is even more liable to cut the wires than the laying of the round wires in opposite directions, for in the latter construction the wires are laid over three sharp edges of the core. Usually in this construction smaller wires are laid over the △ core, but as these smaller wires are taken into account in calculating the breaking strain of the rope, cutting them materially reduces the strain.

Dr. Moir is to be thanked for the experiments he has made in regard to galvanised wires, the results of which were submitted to us at the last meeting. If galvanising the wires of a rope will protect those portions of the wire where the galvanising cannot be removed by wear, we might be able to deduce some rule of finding the breaking strain of a worn rope. In British colliery work galvanising is a very great success,

and it should be given a fair trial in wet shafts on these fields.

In regard to the experiments made by Mr. Heymann, and Dr. Moir's remark about the "showing up" of the Crown Reef water, it should be borne in mind that wires placed in this water, for the same time as Mr. Heymann made the experiment with dropping water (48 hours), will be entirely eaten away. This is probably due to the greater amount of acid acting during the same time. The fact remains that wires of ·061 diameter placed in this fall of water for 48 hours will be entirely dissolved, and water such as this is very disastrous to ropes should it reach them.

One thing which the authors have not made any experiments to prove, but which is a question of great importance to engineers in charge of ropes, is: Has the entire length of a rope suffered damage by having been strained nearly to its breaking strain or broken in one place? The writer is of the opinion that a shock does not damage the rope unless it has broken a number of wires or damaged the strands. Some time ago the writer made some tests of the wires of a rope which had been broken in a testing machine. The tests were made as nearly as possible to the breaks, and in all cases on the portions of the wire which had been under strain. The construction of the rope was six strands of 37 wires each, and the quality special improved plough steel. The results were as follows: Original average breaking strain of wires, 316 lbs. Average breaking strain of wires after being strained, 306 lbs. Loss in strain, 3·1 per cent. The wires gave an even test, and none were very much more than this percentage below the original strain. The average torsions of the original wire were 74·2 in 8 ins. The average torsions of the wire, after being subjected to the strain, were 58·6, thus showing a loss in torsion of 21 per cent. The right angle bends were the same before as after the strain. These tests would indicate that it is perfectly safe to work a rope that has been severely strained or one which has been broken by an overwind or other accident. If the authors could see their way to making some tests along this line and would embody them in their reply to this discussion, these tests would be of great interest and value to those of you who have charge of wire ropes.

In closing these remarks, which quite inadequately take up the subject matter of this very valuable paper before us, the writer wishes again to add his personal appreciation of the merits of the paper and his thanks to the authors.

Mr. W. Martin Epton: We have made one or two tests of ropes that have broken in shafts.

The President: I should just like to add one thing respecting that particular rope Mr. Thomas refers to. I can confirm all he says with regard to the dimensions and position of the fracture. One hears sometimes that a rope working on the underlay of the drum has a worse chance of standing the wear. In this particular instance it was the rope working on the overlay of the drum which we had the most trouble with, and it was that which failed.

Discussion adjourned.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. Vaughan and W. Martin Epton (*Members*).

CONTINUED DISCUSSION.

Mr. T. Pollard (contributed): There recently came under my notice a hauling rope 1⅛ in. diameter, which had a guaranteed breaking strain of 62·7 tons, or on the load hauled a factor of 9·6. There was some doubt about the section of wires being able to give this load, and a portion of the rope was tested, when the breaking strain was found to be only 47·88 tons, or a factor of 7·3.

The suggested explanation is, I hope, the correct one, viz., that the ropes had been mixed up in transit or in the store, and an inferior quality rope issued by mistake. This rope only lasted about six months. I have found in a compound shaft the rope next the splice or skips to be so damaged that about 80 ft. had to be cut out every month. These ropes were of best possible make. I conclude that the fault lay in faulty design of rollers in the bend. These were of cast-steel, only 12 in. to 15 in. in diameter, and very heavy, so that the rope in coming in contact with the wheels had run this distance before peripheral speed of wheels attained speed of rope.

A wire rope dressing which I have found to give satisfaction is composed as follows:

4 drums Stockholm tar, 240 lbs.
1 drum tallow, 56 lbs.
20 lbs. black-lead.

This, boiled and applied hot to a dry rope, will last for several months, and costs about 4d. per lb. I have tried some patent dressing at 1s. per lb., which is recommended for its searching qualities, it being claimed that it finds its way into the heart of the rope. This I do not doubt, as after a week there was none left on the outside.

THE PRESIDENT (Mr. E. FARRAR): The point raised by Mr. Pollard with regard to the pulleys taking so long to get up to the speed of the rope, that the rope is worn in the meantime, is quite an important one. We find the same thing, though to a smaller extent, in headgear pulleys where the rope has anything from 60 to 120 deg. lap on the circumference. Something might be done to lighten these pulleys or increase the diameter, or both.

Dr. J. MOIR: I am sorry that my absence from the last meeting deprived me of the privilege of hearing Mr. J. R. Williams' inspired remarks on my contribution. I do not know whether Mr. Williams meant his remarks to be taken seriously or not, but if he did mean what he said, I must enlighten him somewhat.

(1) Sulphuric acid has a very strong affinity for water, which shows itself even in dilute solution by a diminished vapour pressure, so that the solution evaporates with more difficulty than water.

(2) In a *damp* shaft there cannot be any appreciable evaporation, consequently no appreciable concentration of sulphuric acid could take place.

(3) Even if the shaft were absolutely dry, there could only be a concentration of about 10 per cent., so that water originally containing ·020 per cent. of acid might eventually contain 0·22 per cent. This would not make any difference.

(4) It is not possible to concentrate sulphuric acid to 100 per cent. by any process of evaporation whatever.

(5) It is grossly unscientific to assert that sulphuric acid concentrates itself appreciably in a shaft, unless one has performed some experiments in support of such an unlikely occurrence.

Mr. Williams reveals himself in his true colours when he says, "What we want is commercial science." I can only say that it is difficult to estimate how much progress in the industry has been retarded by such views.

Some of the other speakers appear to think that I meant to assert that my method of galvanising improves the wires, which is far from being the case. The method was adopted merely because it was the only way of securing that the steel of the wires should be chemically the same, otherwise the comparison would not have been accurate. I should like to point out that the sole object of my experiments was to see if there is anything in the theory of the galvanic protection of iron from corrosion, and the experiments have decided in its favour. Had the experiments gone against the theory, then there would have been no necessity for costly trials of commercial galvanised wire in shafts. As it is, these trials are now rendered desirable, as they are almost certain to be successful.

This is exactly the sort of thing in which preliminary scientific work is indispensable, viz., the prevention of waste of money on large-scale trials of the ideas which are likely to arise from the half-knowledge of the "commercial" scientist.

THE PRESIDENT: I notice that Dr. Moir does not make any reference to the effect on the tensile strength of the wires in galvanising. The test of those wires showed a very slight increase of tensile strength, but that, I think, is not the experience of most people who have had to do with galvanising. I should like to know whether he considers there would be an increase or a decrease in tensile strength due to galvanising.

Dr. J. MOIR: I tried to explain that I was forced to adopt that method of galvanising, and I do not contend that it really improved the wire. In order to carry out the galvanising of the wires they were heated in melted zinc to a temperature of 450 deg. Centigrade, and then suddenly immersed in cold water. This rendered the wires extremely brittle, and that is why they did not stand the torsional test, and why the tensile strength was increased.

THE PRESIDENT: The difficulty at present, in the case of deep-level shafts, is to get a rope strong enough to bear its own weight in addition to the load it has to lift. If galvanising is not going to increase its

strength, it is no use increasing its life. The preponderance of opinion is, I think, in favour of the strength being reduced by galvanising.

Mr. H. Leupold: I should like to ask Dr. Moir if he ascribes the increased strength of this wire to the increase in section by the zinc added, and whether he does not think the tensile strength of the steel itself is reduced in commercial galvanising, where only a very slight coating of zinc is used? I should also like to ask Dr. Moir whether he does not think it possible that a very thin film of diluted sulphuric acid might be concentrated appreciably in the two or three minutes that a rope travels up a shaft. In the case of a quantity of diluted sulphuric acid having a depth of $\frac{3}{4}$ in. or $\frac{1}{2}$ in. the same quantity of water in proportion would evaporate as would evaporate from a very thin film, or would the concentration soon attain a point where it would cease?

Dr. J. Moir: The point I wish to make clear against Mr. Williams is that in the shafts I am acquainted with saturation exists to within a few hundred feet of the surface, and consequently all evaporation stops. I am pretty sure that unless you have 1 per cent. of sulphuric acid, you get hardly any action on iron. The recent experiments with nascent hydrogen show that it really is the cause of deterioration, owing to its alloying with the iron. With regard to Mr. Leupold's first inquiry about the increase in tensile strength of the wire being due to the effect of the thick coating of zinc, I think his explanation must be the correct one. In addition to this, there might be an increase owing to the sudden cooling.

The President: I think the explanation as to the wire having been cooled in water at that temperature possibly accounts for a certain increase of strength. I might point out with regard to the rope travelling in the shaft that although the air in the shaft may be at saturation point, yet the rope has to pass from the mouth of the shaft on its way to the drum, and the dryness of the atmosphere may have an effect at that distance, although it would not in the shaft itself.

Discussion adjourned.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. Vaughan and W. Martin Epton (*Members*).

CONTINUED DISCUSSION.

[*Extract from a letter by Mr. Wildy, Managing Director of the Kamfersdam Mines, Limited, addressed to Mr. Robt. Whyte, of Messrs. Allan, Whyte and Co., Glasgow, and dated London, 25th September,* 1905.]

"Very many thanks for sending me for perusal the reports of proceedings chronicling the two papers on wire ropes and the discussion.

Both papers are excellent and add greatly to our information on a very important and interesting subject. They also by their technical and theoretical nature provide material for considerable thought and study, and I am sure that not only the Johannesburg Society, but the mining and engineering communities throughout the world are indebted to the authors for the papers, to those who added so materially to our knowledge of the subject by contributing to the discussion, and to the Society for the excellent reproduction of the plates to illustrate the subject matter of the papers. The chemical question is most interesting, and I was not surprised to find that the scientists differ. I, from previous experience and also from the results of tests given in the paper, adhere to the CO_2 theory and the dissociation and reassociation producing a sort of mutual exaltation of the oxidising process as a practical common-sense argument. The H_2O_2 theory may, under certain circumstances, account for some of the destruction which takes place, but CO_2 appears more practical. There is an example here in London just now of a big bridge with girders 20 ft. deep, with plate webs, and used for a railway. The lower part of the webs is deeply oxidised on the inside, doubtless due to the presence of an excessive amount of carbonic acid in the atmosphere settling in the deep trough, and the alternate wetting by rain and drying has produced just the very best conditions for oxidation by ferrous carbonate.

I was very glad to see the testimony to the value of your pet theory of galvanisation as a preventative of oxidation. Most

men say at once, "Oh, the very thin skin of spelter will be worn off very soon," failing to realise that the greater mischief to a rope takes place—oxidation being once set up—on the hidden inner wires on which the friction to remove the skin is very slight. The report on your galvanised hauling rope is satisfactory, but it would have been more so if it had been stated whether the lead on to the drum was to the top or bottom. That alternate bending stress on one side and then the other, with the under-led rope, must be much more destructive to a rope than where the pit-head sheave and the drum bend the rope in the same direction. If your rope was led under the drum, then the 25 per cent. increase of life would be equal to quite 50 per cent. on the top lead.

I have posted the journals to you to-day, and many thanks for affording me the opportunity of reading and studying the papers."

THE PRESIDENT (Mr. E. FARRAR): There is a point with regard to this under-led rope as to whether the wear is increased, as is sometimes supposed by a reverse bend. When you take the twisting of the rope into account, you do not know whether it is bending backwards or forwards.

Mr. W. C. DOCHARTY: A remarkable instance came to my notice in this connection. Some two years ago we started a little shaft in which we put two ¾ in. ropes driven by a small motor. The rope that was led to the inside of the drum we had to replace in nine months. The other rope leading on to the other side of the drum lasted 15 months. Presumably the ropes were both the same.

Mr. H. J. S. HEATHER: What was the distance from the drum to the headgear sheave?

Mr. W. C. DOCHARTY: I should say about 100 ft. in both cases.

Mr. J. A. VAUGHAN: Was the bending strain what you call an excessive one? As far as Mr. Epton and myself were concerned, to a great extent, we can only go on information supplied to us. In our experience we have not met with this shorter life of winding ropes due to their winding on the under-lay, though, of course, in all treatises on the subject it is reckoned that the rope which has this extra bend has a shorter life. On these fields we know of ropes which have 8 or 10 reverse bends, and which give in some cases a good lift all the same.

THE PRESIDENT: There is one case I know of, and which has been referred to here before. This was where two ropes were supplied by the same maker at the same time. One rope gave out in three or four months, but in this case it was the one on the top-lay which went, and not the bottom one, which rather contradicts the theory of the reverse bend.

Mr. H. J. S. HEATHER: The question of the reverse bend comes in very plainly in the case of flat ropes. When using flat ropes you know exactly which way the rope is bending, and I never found, so far as I could detect, any difference in the life of the ropes which were bent.

Mr. H. H. JOHNSON: I can bear out Mr. Heather's experience. We had flat ropes on the Village Main Reef, and at one time we had a theory there that if we gave the rope a half-turn we would get another six months' wear out of it. As a matter of fact, we never got five minutes' more wear out of it. There is one instance of a pair of ropes which Mr. Epton ran on a hoist operating in an incline shaft without any headgear. The over-lay rope went straight down the shaft from the drum, and the under-lay rope had to go over a guide pulley. At that time we only got about five months' wear out of the under-lay rope. It was giving us a lot of trouble, and we increased the size of the pulley from 3 ft. to 6 ft., and now I do not think there is a week's difference in the wear of these ropes, showing, I think, that there is not much in the "reverse bend" idea. I think there is a good deal in what the Chairman has said about the twisting of the rope, and if there is anything like a decent length of rope from the headgear sheave to the drum it is quite likely we get some turning, and I should like some member who has the opportunity to arrange for a paint streak to be marked on the rope and see if this twisting does take place.

Mr. J. A. VAUGHAN: I think it is more a question of whether the bending stress is a large one or not. If it is a large stress I fancy you will find that the effect of the double bending is noticeable, but if it is a small stress, then it will not be noticeable in the life of the rope. One special local circumstance that tends to reduce the life of a rope did not perhaps get sufficient notice in our paper. In our experience we

find that the ropes which coil in many layers of the drum have not only a bad effect on the flanges of the drum, but on the life of the rope itself, and if our resident engineers would speak of their experience in this matter, I am certain they would do the consulting engineers a service.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

CONTINUED DISCUSSION.

The following contribution by Mr. W. A. Caldecott was read by the Secretary:—

The references in Messrs. Vaughan and Epton's valuable paper to the influence of absorbed hydrogen upon the toughness of steel wires are of great interest, especially in view of the relatively slight amount of attention hitherto paid to this factor by most investigators, with the exception of Howe.

The importance of the question is, perhaps, increased by the fact that no apparent change is visible on inspection of a steel wire whose physical properties have been profoundly altered by internal changes of structure through absorbed hydrogen, whereas in the case of ordinary external corrosion the fact is apparent at a glance. I have had many tests carried out upon the subject, of which a few may be quoted to illustrate one or two interesting facts in a field of research much needing further investigation. I am indebted to the courtesy of Mr. Wager Bradford and of Mr. W. T. Hallimond for the supply of wires for testing.

The apparatus used for the bending tests given was the ordinary standard form, with two curved faces of ¼ in. radius; the wire was bent back and forward, so that the traverse of 180 deg. counted as one bend. The torsion test was done on an 8 in. length of wire suspended vertically, and supporting a 61 lb. weight; one complete revolution of 360 deg. counted as one twist. All wires were cleaned by caustic soda solution before immersion in the acid solution.

When steel wires, unprotected by a preservative coating, are in contact with a solution of free sulphuric acid, even though extremely dilute, as in the case of acid mine water, sulphate of iron is formed, and the hydrogen liberated is partially absorbed or occluded by the metal. The result is that after immersion in the acid solution less than one-quarter of the number of twists may be required to break the wire as compared with untreated wires, although the wire has undergone no appreciable corrosion and the tensile strength is but little affected. Wires made of "Delta metal" or of "silicium bronze" do not show any special brittleness after immersion in dilute acid.

The following illustrates the number of twists required to break 8 in. lengths of wires 0·101 in diameter of "best plough steel," taken from an unused rope. Some of the wires were tested without acid treatment, and others after immersion, for the number of hours specified, in a dilute sulphuric acid solution, maintained at 0·03 per cent. H_2SO_4 to 0·04 per cent. H_2SO_4:—

	Average
Twists to break untreated wires, 31 30·5, 33·5, 31, 31, 32	31·5
Twists to break after 14 hours' immersion, 13, 12, 11	12
Twists to break after 18 hours' immersion, 4, 6	5

Some new wires, 0·1008 in. in diameter, were immersed for 24 hours in acid water from the shaft of a mine on the Central Rand, which had the following composition:—H_2SO_4 ·0086 per cent., $FeSO_4$ ·018 per cent., $Fe_2(SO_4)_3$ ·022 per cent.; 5 in. lengths of the wire required 22, 21 and 20 twists respectively to break before this treatment, and 14, 15 and 14·5 twists to break after treatment; also, on an average of four tests in each case, 21·6 bends were required to break the wires before immersion as above, and 9·8 bends after.

Boiling acid-treated wires in water for three hours apparently restores the toughness so far as the torsion and bending tests are concerned. For instance, new wires as above described, which had been reduced from an average of 31 twists to 13 twists by immersion in dilute acid, required after boiling in water for two hours the following number of twists: 36·5, 51·5, 31, 25; average 31. Similar wires, reduced by acid treatment from 31 twists to 4 twists, required after three hours' boiling in water the following number of twists: 32, 35, 29;

average 32. Similar new wires, which had been reduced from an average of 23 bends of 180 deg. to 10 bends by eight hours' immersion in 0·025 per cent. H_2SO_4, required the following average number of bends before breaking, after the periods specified of boiling in water, rendered just alkaline with caustic soda:—

15 min.	15·0 bends.
30 ,, ...	17.0 ,,
45 ,, ...	20.3 ,,
60 to 120 min.	22·0 ,,

Boiling as above is but a form of annealing, which might possibly be carried out under working conditions in other ways, as, for instance, by passing an electric current through the wire so as to heat it up to the desired temperature. Such treatment, if practicable, would dry the rope and core, and allow of the thorough penetration of the preservative, which at the temperature of boiling water should be quite fluid. It is evident that no form of annealing can be employed for a rope which would injure the hempen core, and hence measures appear to be limited to lapse of time and temperatures up to 100 deg. C.

New wires of good quality show remarkable uniformity under test, but similar wires which have been acid treated and then heated to the boiling point of water are more regular, as the results quoted show, probably because the hydrogen is not uniformly expelled. New wires under the torsion test show a smooth fracture, whereas acid-treated wires show a ragged fracture, with separation of the wire fibres at this point. The non-homogeneity of wires is shown by the fact that longitudinal crevices develop in new wires by treatment with fairly strong, say, 10 per cent., acid. If wires are twisted before such acid treatment, spiral crevices develop, thus showing that the lack of uniformity in the original material is perpetuated in the wire drawn from it. The etched-out portions may possibly indicate the presence of steel with less carbon acting as an anode in a galvanic couple, whereas the portion of the wire higher in carbon acts as the cathode.

As might be expected, the conditions most favourable to rusting, or corrosion by oxidation, of steel in contact with an acid solution do not promote absorption of hydrogen, since the hydrogen evolved tends to combine with the oxygen present to form water. The above conditions are fulfilled when a wire is suspended vertically in the air, and an exceedingly small stream of acid water allowed to trickle continuously down its length.

Lapse of time also in a great measure restores toughness by allowing the absorbed hydrogen to escape, but its complete expulsion is a most difficult matter; in fact, Prof. Arnold informed me that after heating a sample of steel to redness for several hours it still continued to evolve hydrogen gas. Some new wires as above described, which required 31 twists on an average to break before treatment with dilute acid, and 13 twists after treatment, were buried in lime to prevent rusting for three days and a half; after this lapse of time they required the following number of twists to break: 26, 31, 31, 34, 26·5; average 29·7.

The conditions most favourable for absorption of hydrogen by steel wires in hoisting ropes exist in a mine where preservatives are irregularly used, and where the acid water present saturating the hempen core acts on the wires which are near the centre of the rope and thus partially protected from the air. Conversely, the regular use of preservatives free from acid upon wire ropes from the start retards, in a great measure, both the external and visible, as well as the internal and invisible, deterioration of the wires composing the rope.

Reply to Discussion.

Messrs. J. A. Vaughan and W. Martin Epton submitted their reply to the discussion, which was read by Mr. Vaughan, as follows:—

The authors have already expressed their high appreciation of the honour conferred upon them by this Institute, not only by reason of the great attention their paper has received at the hands of the members, but also that it was thought worthy of the first Gold Medal Award.

In replying now to the friendly criticisms that their paper has evoked, they wish to once more place on record their great indebtedness, and to assure their brother members that the investigations they have been engaged in, and which they are still pursuing, will be continued with renewed vigour, feeling as they do that regarding the subject matter of their paper there is much yet to be learnt, and that the sympathetic and cordial appreciation of all engineers on these fields awaits the discoverer of any

new fact, or the person who honestly and conscientiously propounds any new theory regarding the practice with respect to wire ropes for winding.

Before proceeding further, a personal explanation should be made to the effect that, owing to the pressure of work and to the fact that my colleague had to proceed on leave of absence before the final terms of our reply could be decided upon, this reply is, necessarily, of a somewhat disjointed character, and quite unworthy to be classed along with the carefully considered criticisms that have been delivered.

to buckle out between the spirally laid wires." He asks if we have noticed the modulus appreciably altered in the core wires of an old rope. In answer to this question, it should be first said that it very seldom happens that hard cores are put in the strands of a 6/6/1 rope construction or in a compound rope of, say, 7/5/1 or 8/6/1; so that, generally, where hard wires are used for the strand cores, they are small (·040in. and ·061in. diam.). Previously, no note had been taken of their extensions under test, but Mr. Schweder, having inquired regarding the modulus of elasticity,

TABLE A.

TESTS OF CORE WIRES OF OLD ROPES IN COMPARISON WITH OUTSIDE WIRES.

Number of Test.	Length.	Diameter.	Elongation.	Breaking Load in lbs.	Number of Torsions in Length of 8 in.	Number of 180 Bends over ¼ in. Radius.	Remarks.
	in.	in.	in.				
106	10	·066	1/8	1,020	—	6	Outside wire
106	10	·100	1/4	1,860	19	4	Hard core wire
284	10	·083	11/32	1,480	10	9	Outside wire
284	10	·082	1/4	1,200	4·5	9	,, ,,
284	10	·080	9/32	1,320	22	7	Hard Core Wire
284	10	·080	5/16	1,340	17	7	,, ,,
196	10	·108	3/32	1,960	6	4	Outside wire
196	10	·107	11/32	2,020	—	6	Hard core wire
234	10	·087	1/8	1,560	4	7	Outside wire
234	10	·098	3/8	1,620	—	6	Hard core wire
7	10	·107	3/8	1,660	22	5	Outside wire
7	10	·108	5/16	1,760	27	7	Hard core wirr
41	10	·105	1/4	1,800	3·5	7	Outside wire
41	10	·121	1/4	1,740	8	5	Hard core wire
67	10	·084	1/4	1,700	4	7	Outside wire
67	10	·105	5/16	1,960	20	7	Hard core wire
68	10	·082	3/16	1,700	2	6	Outside wire
68	10	·103	1/4	1,960	11	6	Hard core wire

Note.—The core wires were originally approximately of the same size and strength as the outside wires.

Proceeding now in execution of their task, the writers wish to place before you the following observations:—

Mr. Schweder in his remarks, says he does not agree with Prof. Hrabak with regard to core wires being gripped, but is of opinion that "the core is simply outstretched by the first load application, and when the rope and the wires in the strands creep back, the wire core, which has to go a longer distance, will find sufficient room

further tests have been made, and the following Table A of results is submitted. The time at disposal and other circumstances did not allow of the modulus of elasticity being determined, but the elongations were measured, and are here quoted. It will be seen that the core wires do not appear to have deteriorated, although the ropes have had considerable usage.

If Mr. Schweder is correct in his surmise that the core stretches and then buckles out

amongst the spirally laid wires, then in the case of a rope with a large, soft core wire, which stretches something like 1¾in. in 10in., one would think that some evidence in the way of marking would be visible on the core wires of an old rope. This, however, is not the case. In fact, the marking of wires points in exactly the opposite direction, namely, that the core wire has been gripped so that it cannot move.

Mr. Schweder says it would be a grand thing if it were possible to prove or disprove by actual tests Prof. Hrabák's theory *re* bending stresses. This the authors of the paper would very much like to accomplish, and they are devoting attention to it.

The authors do not consider the investigations on bending stress to be complete as yet. It appears to them possible that the bending stress not only varies with the ratio D/d, but also with the rapidity and frequency of the bending, as also with the extent of the other loading. With a rope under load the strands slightly untwist, and the direction of the wires becomes more nearly parallel to the axis; furthermore, the release afforded to the wires by the compound construction of the rope must be materially lessened by the closer packing of the structure at a time of heavy loading. How is it that, if Hrabák's theory is correct, with the old generally unfavourable conditions of D/d on these fields, wire breaks due to bending did not occur more frequently? Again, in the case of a certain Whiting hoist on these fields, where the ratio D/d, being quite large, and the bending stress figured merely on this ratio quite small, but where the other loading, due to differential stress, etc., is large, wire breaks occur quite frequently, the appearance of the breaks leading one to suppose that the bending stress was the final straw. For these and other reasons the writers consider that it is very necessary to try to evaluate the true stress due to bending by means of practical trials.

The idea of the writers is to take wires, strands, and ropes of various sizes, and to measure the force necessary to bend these samples over curves of various radii. Following this they will endeavour to measure the force necessary to bend the strands and ropes when these are subjected to further loading statically. If it can be found out to what extent the flexibility decreases owing to the tighter packing of the spirally wound wires under tension, some opinion may be formed as to the constancy or otherwise of the bending stress.

Mr. Schweder's final criticism regarding the loss of strength in a winding rope, due to the fact that some of its wire section is worn away, is practically answered by himself when he alludes to the higher modulus of elasticity of worn ropes as discovered by Hrabák. To secure safety, it will certainly be better to take the writers' advice and consider that 25 per cent. loss of wire section means 25 per cent. loss in the strength of the rope.

Mr. Cowell remarks that the torsions of wires of over 135 tons ultimate stress, as given in Table I. of the original paper, are very low. The torsions there stated are in each case the average of the results obtained from all the wires in two strands of the ropes. The worst feature about some of these is their variability. On a further perusal of the details, it is thought that the following additional information will prove interesting:—

Torsions of wires of rope	No. 92	varied from	6—51
,, ,,	No. 113	,,	9—59
,, ,,	No. 114	,,	10—50
,, ,,	No. 147	,,	4—50
,, ,,	No. 33	,,	3—27
,, ,,	No. 83	,,	5—20

In this connection it is well here to allude to the discussion that has taken place regarding the very guarded statement of the writers that while it was true that high tensile efficiency can only be obtained at the sacrifice of toughness and ductility the extent of the sacrifice was not so great as some people supposed. This the writers backed up by some quoted opinions, and also by the results of actual tests set forth in Table I. Mr. Bullivant and Mr. Thomas attacked the writers from opposite sides, the former gentleman speaking against the reliability in the matter of ductility of high tensile strained wire, while the latter gentleman gave it as his opinion that ductility was not greatly sacrificed. There the matter must be left for the present, for the writers have not been able to carry out more extended trials. The wires from a new rope are the necessary requirements for this experiment, and it is not often that a sample of new rope is submitted for test. Samples of wire are readily obtainable, but not so a sample of a new rope of the requisite highly drawn material. The case quoted by Mr. Thomas showed considerable

want of uniformity, the torsions varying from 28 to 45, although the average was quite satisfactory.

Mr. Cowell suggests that the Government Mining Engineer's Department should compel rope manufacturers to adhere to a standard specification. This matter will doubtless be dealt with by the Transvaal Government Commission which is now sitting to consider the safety of winding in shafts. With regard to the 10 per cent. reduction to allow for "spinning," this was suggested as a reduction from the strength as assessed from the effective sectional area, this latter not to include the core wires if they were soft, but only in case of their being of about the same tensile strength as the other wires in the rope (see note at bottom of Table II).

With regard to Mr. Cowell's criticism regarding the method of fastening the end of the rope specimens for test, the authors have tried many other methods besides the white metal seal, but have found none so good. Where the rope specimen breaks well clear of the dies, it must be apparent that the method by which the specimen is gripped has no effect on its proved breaking load. The reliability and efficiency of the method of fastening alluded to can be noticed, especially in the case of a test of a worn rope. The writers will, however, be very pleased to test, if practicable, any method of fastening suggested by Mr. Cowell.

Mr. Tregaskis (p. 233) says that in the case of a rope wearing all on one side the measurement of the individual wires in the rope and calculating the strength of the rope, as suggested in the paper, would not be correct. This, of course, is quite true. but it is only under exceptionally unfavourable circumstances that a rope wears in this manner, and the authors had no intention of applying the rule to cases of this kind. The other point, raised by both Mr. Heather and Mr. Tregaskis, with regard to the wires "spreading," is well known amongst engineers, and it is evident that on thorough examination revealing this defect due allowance should be made for it. Mr. Tregaskis is to be thanked for calling attention to the evils that may arise from allowing the ropes, when not being used, to remain hanging in wet shafts. This practice must lead to increased deterioration, and should have been noticed by the writers.

Mr. Johnson wishes to know why, if the test of a short length of rope is as satisfactory as that of a long one, the Government should saddle this country with the cost of a new testing machine 20ft. long. Apart from the fact that it is always best to prove the absolute truth of one's opinions, founded on no matter how sure a foundation, the chief reason for the new order was that it had become necessary to obtain a machine capable of breaking ropes of a much greater ultimate load than the one at present in use (50 tons of 2,240lbs.). As the difference in cost between a machine that could take in a 5ft. length and one that could take a 20ft. length was not considerable, and, moreover, as the new machine will be used for tests of specimens other than ropes, it was considered advisable to order the horizontal machine with a long base.

With regard to the great reduction in the factor of safety of winding ropes when the bending stress is taken into consideration, it should be explained that these results were worked out from Prof. Hrabak's tables, as shown in the original paper in Diagram 2 in the form of a curve. Prof. Hrabak up to the present may be considered the best authority on the subject of bending stresses in wire ropes. When any sounder theory is established, the authors will not be backward in adopting it.

With reference to the bending tests of wires, and to the statement in the original paper that "The great difficulty lies in the fact that the breaking stress of the material decreases as the number of applications increases," the context should show that it was not intended to lay down in these words that a constant rate of variation had been discovered to exist. The statement immediately following, to the effect that the formula $\frac{\text{ultimate stress}}{\sqrt{\text{number of bendings}}} \cdot \frac{D}{\delta}$ was found to give a better basis of comparison, appears to state clearly the writers' meaning. The actual stress produced in the outside fibres of a piece of wire (say, ·125 in. diam. $=\delta$) when it is bent over a radius of $\frac{1}{4}$ in. ($D=0{\cdot}5$ in.) cannot be calculated, for the formula stress $= E\frac{\delta}{D}$ only holds goods so long as the elastic limit is not exceeded, which. of course, it is in this case enormously. Further, we do not know the E, or even the ultimate stress, of the outside fibres which

compose the hard skin of a highly drawn wire. Only the average values for the whole wire are known. The large stress produced by this severe bending does not immediately break the wire. The question then arises, how many applications and reversals will do so? The number of bendings before rupture of different gauges of wire has been determined by the writers, and, as shown in their paper, this number appears to bear some fairly close inverse relation to the square of the diameter of the wire. Many more experiments, however, will have to be made before any definite law can be laid down.

The authors take it as a great compliment that Messrs. Denny should have given the paper such full consideration, and should have gone to the length of proposing that the authors should answer their twelve questions; still, they are afraid that many of them must remain unanswered for some time to come, and several of the questions are quite worthy of a complete paper in reply.

Nos. I. and III., referring to $\frac{D}{\delta}$ and the lead of the rope from drum to pit head sheave, will be answered when the bending stresses in wire ropes are thoroughly understood, which up to the present the authors venture to say is not the case. Prof. Hrabak has advanced this subject more than any other investigation, and until we know more we must be guided by his figures.

Regarding Nos. II. and V., which refer to the attachment of the rope to drum and skip, the authors hope to make several experiments on this subject as soon as the new testing machine arrives, as the machine at present in use in the Mechanical Laboratory will not admit of a sufficiently long specimen being tested. It may be said that many experiments have been made on this subject in England and other countries, and the efficiency of many of the time honoured cappings have been seriously called into question. In a few months, however, the authors hope to settle this question, so refrain here from quoting the results previously obtained elsewhere.

Regarding question II*a*, dealing with the crushing effect of "riding turns" of rope, and which really deserved a number all to itself, the authors have on several occasions been called on to inspect damaged ropes which have been worked under those particularly severe conditions, viz., coiled on a parallel drum with several riding layers. A properly turned groove is certainly the best bed in which to coil a rope, and most probably a layer of turns of itself is the worst. In the authors' experience the ill effects are intensified under the following conditions:—

(*a*) When the working tension in the rope is more severe.

(*b*) When the rope is of compound construction, this leading to the use of smaller wires and the production of a rope with less resistance to crushing without deformation.

Regarding Question IV, the bad effect on the rope by using heavy sheaves and guide rollers was pointed out in the original paper. The authors have, from personal observation, proved that considerable slip does occur between the pithead sheave and the rope. This may be called a plus effect on the down trip, and a minus effect on the up, these amounts balancing one another more or less, as in the case of the slip of the Walker ring on the Whiting hoist. The writers consider that the great and rapid wear of the flat rope at the Robinson Deep No. 2 shaft in 1904 was caused by friction between the sheave and rope, this being plainly shown by the scoring of the tread in the sheaves. It is obvious where high speeds of winding are in vogue that this deteriorating effect should be watched for.

With regard to Question VI., if reference is made to Table IV of the paper it will be seen that the authors have made some tests of rope dressing in use. Some of the points of importance that it is necessary to observe are there noticed. The staff of every mine and the appliances at hand will suffice for a test of this nature to be readily carried out. The conditions are not the same on every mine, and no general rule can be laid down excepting with regard to the non-acidity of the composition. It is obvious that on each mine the rope dressing should be tested as to its suitability.

Regarding Question VII—rope deterioration, evidence of, and danger signs—a considerable portion of the paper was devoted to this. Reference, however, may be made here to a paper by Herman Kroen on "The Allowable Number of Broken Wires in Ropes used for Hoisting Men." He quotes Roch, who says: "The worst portion of a rope may be considered that portion which in a length of ten times the lay shows most wire breaks. In calculating the remaining

strength of the rope, one wire should be deducted for each break observed." Kroen considers that Roch's suggestion gives an unnecessarily large limit of safety, but adopts it in his further calculations so as to be on the safe side. He points out that in a six-strand rope the visible wire surface is to the invisible as 1 : 0·5, so that the total number of all the broken wires in any given length of such rope is equal to one and one-half times the number of visible broken wires. If Z^1 represents the allowable number of broken wires in ten lengths of lay, and if Z equals the number of visible breaks in one foot-length, and if p equals the pitch in feet, then

$$Z = \frac{Z^1}{1{\cdot}5 \times 10 \times p.}$$

By combining this formula with that previously given for the factor of safety of the rope, Kroen obtains a formula which states the allowable number of visible broken wires per foot run, viz.,

$$Z = \text{or} < \frac{(F - 6{\cdot}5) \times n}{15\ p\ F}$$

[Note.—This formula is for six stranded rope; F denotes the factor of safety of the rope when new, and n denotes the number of wires (neglecting soft cores) in the rope.]

Kroen then works out a few cases from the data furnished of several ropes actually in use, and finds that about nine broken wires per running metre could be allowed. It should be noted, however, that in these cases the ropes were of compound construction—the wires being small—and the ratio $\frac{D}{\delta}$ was high, somewhere about 2,000.

As against all this theory, the results of some practical tests may be quoted. A recent case of a slightly worn rope gave the following results: The rope was 1¼ in. diameter, constructed of six strands, each containing twelve wires (·091 in. diam.) round twelve wires (·054 in. diam.) round triangular core. The specimen of whole rope tested contained eleven broken outside wires in a length of 23½ in., and broke at 116,928 lb., while the mean breaking load of twenty-four outside wires, as tested separately, gave an average of 1,180 lb. The inside wires and core wires were also tested, and from these results the breaking load of the whole rope was calculated to be 138,936 lb. By deducting the eleven broken outside wires we should get a breaking load of 125,800 for the whole rope. The specimen containing the eleven visible broken wires actually broke at a load of 116,928 lb., or under a load of 7 per cent. less than that calculated. It is possible, of course, that some of the inside wires may have been broken, but not at all probable. When the specimen was being broken, single wires started to break at 67,000 lb., and fifteen wires broke in succession before the rope finally collapsed at 116,928 lb. load. Immediately before the collapse there were 11 + 15 (= 26) broken wires in the rope, and its calculated strength would be 108,256 lb., so that regarding the test from this standpoint the rope stood 7·4 per cent. more load than calculated. The writers here present Table B, showing the tests of rope samples after their being subjected to artificial wire breaks. Arguing from all the above data, and taking into account the want of agreement in the results obtained

TABLE B.

Tension Tests of two ropes, each specially prepared by having ten of the wires cut through.

Construction, Lang's lay, 6/6/1; diameter, ¾ in.; length of lay, 7½ in.; length of test piece between grips, 26 in.; condition of rope, new.

Breaking load of whole rope (no wires broken)	38,416 lb.
Breaking load of whole rope (ten wires cut)	31,037 lb.
Mean breaking load of twelve wires = ...	1,042 lb.
Breaking load of hard core wire = ...	1,060 lb.
Aggregate strength of wires = 1,042 x 36 + 1,060 x 3 = * ...	40,692 lb.
Less ten wires	10,420 lb.
Estimated strength =	30,272 lb.

And this proved to be 2½ per cent. below actual strength.

*Rope contained three hard and three soft core wires.

Construction, Lang's lay, 6 of 8/6/1; diameter, 1⅛ in.; length of lay, 10 in.; length of test piece between grips, 26 in.; condition of rope, slightly worn.

Breaking load of whole rope (no wires broken)	95,110 lb.
Breaking load of whole rope (ten wires cut)	84,291 lb.
Mean breaking load of sixteen outer wires	1,600 lb.
Mean breaking load of twelve inside wires	547 lb.
Mean breaking load of two core wires	560 lb.
Aggregate strength of wires = 1,600 × 48 + 547 × 36 + 560 × 6 = ...	99,852 lb.
Less ten wires	16,000 lb.
Estimated strength =	83,852 lb.

Which proved to be true within ½ per cent. Each strand contained a hard core wire.

by different experimenters, the authors do not at present feel inclined at all to recede from the position they took up in the original paper when they stated: "Without further knowledge the writers would recommend that the total number of wires broken in a length of 8 ft. should be taken to mean a corresponding reduction in the strength of the rope."

On the point as to how far an apparent gain or loss of elasticity in a rope may be regarded as a danger signal, the authors have not been in a position to form any opinion owing to the lack of authentic details. If it were the practice on the mines to watch and record data dealing with this interesting question, the correctness of any inferences drawn as to the breaking load of the rope could be readily settled by submitting a sample for test. Speaking generally, it may be said that the testing of worn ropes can only be of the greatest service if every possible piece of information as to the life, conditions of service, and observed peculiarities are furnished at the time the specimen is submitted. This is far from being the case at present, and the writers invite more attention to this point. Mr. Schweder has performed a useful service in drawing attention to Hrabak's practical method of obtaining the modulus of elasticity of new and old ropes.

The three most obvious danger signals in a working rope are excessive wear, broken wires and corrosion, all of which are fairly easily to be discerned if examinations are regular and thorough. In connection with corrosion, the authors wish to state that whereas, as a general rule, internal corrosion is accompanied by very evident signs of external corrosion, this is not always the case, and ropes should be internally examined from time to time. In one case in their experience of very severe internal corrosion there was absolutely no evidence of this action on the exterior of the rope. In another case, one of severe external pitting, there was no damage whatever to the interior. Probably in the latter case the internal lubrication had been well attended to in the process of manufacture of the rope—the reverse, doubtless, of the former case. Both these were instances of hard-worked ropes.

With respect to Question VIII, dealing with the necessity for further regulations *re* winding ropes, this matter may be left to the Government Commission now sitting.

Question IX, *re* sampling water for acid tests, may be left in the hands of the mine manager, who probably knows at which part of the shaft the most acid water is likely to be found.

With regard to Question X, *re* twisting and kinking of ropes: In putting on a rope kinking can be avoided by using due care in taking the rope from its reel, and winding directly on to the drum of the engine. This is usually done, when time and circumstances permit, on mines where the matter is considered of sufficient importance, by mounting the rope reel in bearings, either improvised or, as in some cases, specially provided. A brake of some description should be used in conjunction with the reel. The rope may, of course, be kinked by the driver letting out too much slack when the cage is resting on the chairs, or through a jam in the shaft. If a good driver knows he has done this, he will avoid kinking by having the rigger or some other experienced person below to tend the rope while he pulls in. If, on the other hand, he pulls in blind, damage is likely to occur to the rope. There are other ways in which slack rope may be caused, such as a rope coming off the drum by flanges breaking, etc., etc., but they would be in sight of the driver, and he would notice any tendency to kink. There is no harm in trying to get a kink out by legitimate means which will not damage the wires. The test of success would be the appearance of the rope. Where possible, of course, the kinked portion should be cut off.

The authors wish to thank Messrs. Denny, not only for the full consideration they have given to the paper, but also for their offer to submit all the information with regard to the practice on the mines under their management. It is obvious that if others will do the same a mass of valuable data will be collected, which will be for the benefit of the mining industry, in that the possible defects in manufacture may be recognised, errors in treatment discovered, and a relative value attached to the many deteriorating influences which affect the life of the winding rope. This will all lead to a more intelligent knowledge of the actual reserve of strength existing in a rope, as well as to the necessity for its being there. The arbitrarily fixed minimum factor of safety for all cases may then become a thing of the past, and the expense incurred in securing the best conditions of working may be compensated for by the preferential treatment accorded to the best practice.

To make their meaning quite clear, the writers would direct attention to the manner in which the strength of boilers is assessed by the British Board of Trade, and to the discrimination shown by that body to such important matters as design, material and workmanship. The Board has made special rules in order to ensure that boilers, well designed, well constructed, and made of good material, should be allowed an advantage in the matter of working pressure over boilers inferior in any of the above respects. Starting with a factor of safety of 5, to be used when all conditions are of the very best, additions are made for each instance of bad practice, so that in an extreme case these might total 6, and thus enforce a factor of safety of 11. It would be quite feasible, although perhaps not quite so convenient, to give the preference by means of deductions made from a standard factor established as suitable for ordinary, normal good conditions; but even then it would probably be necessary to enforce additions to this factor in some cases.

Mr. Thomas' criticism of the writers' suggested formula $\left(\frac{\text{mileage run}}{\text{factor of safety}}\right)$ for the comparison of the work performed by various winding ropes at various places does not appear to advance things at all, especially considering the fact that in the original paper it was stated that in arriving at the real factor of safety for this purpose it was necessary to compare the true effective strength of the rope with the total effect of *all* the destructive influences, these latter being pretty freely alluded to subsequently. The most obvious way to take these circumstances into account is by making special deductions on account of each from the nominal factor of safety calculated in the usual way from a consideration of statical conditions only.

Dealing with two special aspects of the case, viz., the grade of steel used in the manufacture of the rope, and the conditions of service in the matter of bending stress, Hermann Kroen (Manager of the St. Aegyd's Iron and Steel Industrial Co., Vienna) has proposed a formula for the minimum factor of safety, such as the following. Statical factor of safety to be—

$$\leq 7 + 0{\cdot}005\left(\frac{b}{142} - \frac{D}{\delta}\right)$$

where b = the ultimate tensile stress of steel in lb. per sq. in., D = diameter of drum or sheave, and δ = diameter of largest wire in rope, both diameters being measured in inches. Using this formula for a specific case, if $b = 284{,}000$ and $\frac{D}{\delta} = 1{,}500$, then the minimum statical factor of safety should be 9·5. This appears a rather severe treatment of the case, but it is put forward here, without necessarily agreeing with Mr. Kroen, just to draw attention to the method by which a nominal statical factor of safety of 9·5 would be reduced to 7 on consideration of two special circumstances. As far as these two circumstances are concerned, this formula points out one way of comparing the factors of safety in use in different winding plants. For reasons such as the foregoing the task of comparing the duty of ropes working in different shafts is not considered so hopeless as Mr. Thomas states it to be.

In referring to the matter of the standard specification of wire ropes, Mr. Thomas suggests that the only alteration that is necessary to the present practice is that some standard percentage of deduction should be made from the aggregate strength of the wires for each of the various rope constructions. This to some extent is in accordance with the views of the writers,

but in consideration of the differences of opinion that exist as to what ductility a wire of high tensile strength should show, it also appears advisable that the guaranteed torsions and bends (either average or minimum as decided upon) should also be stated in the specification by the manufacturer.

In response to Mr. Thomas's question with regard to ropes that have been strained by a big shock, such as an overwind, it is evidently an incomplete answer merely to state that if the wires are not stressed beyond their elastic limit, then the rope would not be any the worse, while if they are strained to that extent or over, then undoubtedly the rope would be permanently damaged—incomplete, because the evidences of strain beyond elastic limit might be slight and difficult to discover any way.

In the case of a wire stressed by tension beyond the elastic limit, permanent elongation and permanent reduction of cross area occur. If the test is continued until fracture occurs, it will be found that the amount of elongation and the amount of reduction in cross-section are far greater in the immediate neighbourhood of the fracture than anywhere else in the specimen. At these latter parts the reduction in area is hardly appreciable, and difficult to measure. If a period of rest is allowed (regarding the duration of which the writers cannot speak), and the two pieces into which the wire has been broken are each separately tested, they will be found not to have deteriorated much in the matter of ultimate tensile stress, while the elastic limit may actually have been raised. The writers will experiment further regarding this effect, and also as to whether the elastic limit is raised or lowered, and whether the capacity to resist bending and torsion is adversely affected. Mr. Thomas's experience in this respect is very interesting, and corresponds practically with that of the writers. The local reduction of area above referred to can generally be seen at the fractured ends of the wires after a whole rope test, and this effect can also be seen in the cases of ropes or bridles of skips that have been broken at times of overwind.

Two interesting tests were made in the Mines Department Laboratory, each of a piece of rope cut from a point near to where the rope had been broken, due to an overwind, and in neither case was a reduction of tensile strength noticeable. More of this class of work will be done as the opportunity offers, but rope breaking by means of overwind is, fortunately, not of frequent occurrence. In looking for a reason for the apparent immunity from damage of the rest of the rope, it must be remembered that the stress due to a shock is transmitted through a wire or rope by a wave motion. In the case of an overwind, it is highly improbable that the initial stress created, owing to the sudden stop, will be sufficient to break the rope, but as this is the case of a short rope, there will be produced numerous super-imposed reflections of the wave, with resulting extra stress. If this is true, then it will be seen that the whole of the rope between the winding drum and the skip will be stressed nearly to breaking point. In all probability, however, there will be no evidence of this except at the point of fracture, and under test the rope may appear as good as ever. It will evidently be the proper thing to remove as much as possible of the overstrained rope and have it thoroughly tested. In the writers' opinion, the portion of rope that was resting on the pithead sheave at the time of the overwind should certainly be cut off.

The matter of how the working wear was distributed in a winding rope was touched upon by at least one of the contributors to the discussion. This is a very interesting point, but unfortunately the subject is a lengthy and a costly one to investigate.

The writers can, however, give you particulars of several tests conducted of samples taken from various selected places in a rope (1⅛ in. diam.) worn to a finish at the Robinson Deep Mine—that is to say, used until the authorities thought it prudent to discard it. The piece they selected for test as the most worn proved to be so, which is very satisfactory, and bears out the authors' contention that careful examination of a rope will reveal a great deal as to the strength remaining in it. This particular rope had a breaking load of 125,440 lb. (say, 62½ tons) when new, according to the maker's certificate, its construction being six strands of nine over three, with

wires ·095 in. in diameter. The full details of the test are as follow:—

Sample Taken.	Breaking Load.	
	lb.	Tons of 2,000 lb.
25 ft. from skip...	94,304·0	47·1
200 ft. ,, ...	84,851·2	42·4
250 ft. ,, ...	72,060·8	36·0
400 ft. ,, ...	82,208·0	41·1
600 ft. ,, ...	83,843·2	41·9
800 ft. ,, ...	81,670·4	40·8
1,000 ft. ,, ...	87,584·0	43·8
1,200 ft. ,, ...	87,696·0	43·8
1,400 ft. ,, ...	88,457·6	44·2
1,600 ft. ,, ...	84,000·0	42·0
1,800 ft. ,, ...	101,763·2	50·9
1,850 ft. ,, ...	107,676·8	53·8

Sample cut 250 feet. from skip showed signs of wear both outside and at the back of wire, and also considerable pitting.

Sample cut 1,850 ft. from skip showed wear on outside of wires, but very little on inside, and no pitting.

The distribution of wear as disclosed in the above test does not argue in favour of the use of a taper rope.

In another instance of a discarded rope the following results were obtained. The rope had been in service for 7½ months, and was taken off because it had got kinked and the bad place could not be cut off so as to leave a serviceable length. The breaking load of the rope, when new, according to the maker's certificate, was 125,440 lb. (say, 62½ tons).

Sample.	Breaking Load.	
	lb.	Tons of 2,000 lb.
End next splice ...	95,827·2	47·9
200 ft. from splice ...	95,782·4	47·9
400 ft. ,, ...	92,848·0	46·4
600 ft. ,, ...	92,268·0	46·1
Near to kink	93,385·6	46·7

The practice of submitting to test samples of a winding rope, when it has been decided to discard it, cannot be too highly commended. It will enable the engineer to learn definitely how far his judgment as to the strength remaining in the rope is correct. Any conclusions drawn as to the duty obtained from a winding rope must obviously be incomplete unless the true strength of the rope, at its most worn part, is ascertained at the time it is taken off.

From knowledge gained in this way the possibility and advisability of using a lower factor of safety for deep level winding may perhaps be discovered. The experiment of using a lower factor than 6 can, of course, only be tried when hoisting rock or water, but where circumstances permit, there is a great deal to be said in favour of the trial.

The authors are more than glad that they had the temerity in their paper to introduce the chemical aspect of rope deterioration. No misconception appears to have arisen owing to the step—the chemists have come forward with alacrity to assist in the inquiry, and they have attacked each other, and not the writers.

The interesting experiments of Mr. Heymann go to show that even the most acid mine water has very little more corrosive action than plain water, and that the primary cause of the corrosion of the wires of winding ropes is contact with air or deleterious gases. This is a most important point, in that it allows of experience gained in Great Britain with respect to the efficiency of galvanised wire rope to be referred as holding true for the Transvaal. Dr. Moir's experiments have entirely disposed of that bogey, which was always a scientific fallacy, viz., that during the life of a galvanised wire rope, when the zinc was worn off in places, the galvanic action would then lead to more rapid wasting of the steel. Through the courtesy of Messrs. Allan, Whyte & Co., Dr. Moir and the writers have been able to repeat the experiments, the results of which were previously published, dealing with the effects of galvanising as a preservative and as a deteriorating agent with respect to the ductility of the steel. The samples forwarded represented identically the same wire, plain and galvanised, so that an exact comparison was possible. Table C shows the results obtained. The method of preparation of the specimens for the chemical test was the same as previously described by Dr. Moir. It is evident that a coating of zinc has a protective action on steel even when the latter is exposed to the acid. Generalising, results show that plain wire suffers about thirty times as much corrosion from fairly strong acid as the same wire does when it has a coating of zinc near the exposed surafce of the iron, and about ten times when the acid is weak.

The galvanising in these instances was, of course, of the ordinary commercial character, and not *home made*, as in the case of the previous experiments, the reason for repeating the tests being that the latter improvised method of galvanising had, it was suggested, possibly spoilt the wire.

The effect on the ductility and on the other desirable qualities that the hard drawn steel originally possessed is fairly clearly set forth in the tabulated results. The elongation is increased nearly 100 per cent., while the tensions and bendings obtained are reduced, the former rather less and the latter in a rather greater degree than was anticipated from the results recorded by previous investigators.

Mr. W. A. Caldecott's contribution read here this evening must be regarded as an interim report published at the earnest solicitation of the writers before the conclusion of the discussion. It makes public the fact that Mr. Caldecott has embarked on a most important investigation, and the Institution to which he decides to present a full account of his researches is to be envied. It is to be noticed that Mr. Caldecott's results obtained up to date agree generally with those quoted by the writers from Marion Howe's treatise. The attention of Mr. Johnson and Mr. Docharty, who apparently did not believe that the ductility could be partially restored by the slow process of simple exposure to the atmosphere at ordinary temperatures, may be directed to Mr. Caldecott's statement that a relatively short lapse of time also partially restores toughness by allowing some occluded hydrogen to escape, but its complete expulsion is a most difficult matter.

In concluding these few remarks and gratefully acknowledging all the assistance they have already received, the writers have only to express their conviction that the great deal that yet remains unknown regarding the winding rope does not involve problems incapable of solution. The resources of these mining fields afford ample scope for the necessary practical trials; a sympathetic Government has provided the Mines Department with an adequate testing laboratory, and, given the cordial co-operation of all engineers and rope manufacturers, perhaps the most necessary item of the whole equipment, and one that it is felt certain will never be withheld, the writers feel assured that future progress towards the settlement of these problems is no longer in doubt.

THE PRESIDENT (Mr. E. FARRAR): I think you will agree with me that the reply to this discussion is second only in value to the paper itself. We have had an addition to our information in this respect, which, taken in conjunction with the paper itself, will largely increase our information with regard to wire ropes. I should like to ask Mr. Vaughan if he would give us the difference in tensile strength between the galvanised and non-galvanised wires he refers to.

Mr. J. A. VAUGHAN: The difference only amounted to a very small percentage. If you take the actual area of the wire, counting in the thin coating of from $\frac{1}{1000}$ to $\frac{1}{500}$ of an inch of zinc on the outside, it shows really a moderate reduction in tensile strength, but where you figure it on the actual area of the steel there is really no appreciable reduction. The figures are a bit variable. We got in one case here 1,200 lb. as against 1,220, the breaking load of the wire, the 1,200 being ungalvanised and the 1,220 galvanised. The makers at home get 1,210 and 1,215. Unfortunately, Mr. Epton, who actually carried out the tests, is away, but I am certain of this, that he was convinced in his own mind that there was practically no reduction in tensile strength at all. He was rather surprised at obtaining such a big drop in the bends, and such a small drop in the torsions as between the galvanised and plain wires.

WIRE ROPES USED FOR WINDING: THEIR STRENGTH, AND SOME CAUSES OF ITS REDUCTION.

Paper by Messrs. J. A. VAUGHAN and W. MARTIN EPTON (*Members*).

Mr. G. A. Goodwin, M.I.C.E., A.M.I.E.E., Wh.Sc., of London, sends the following written contribution. It arrived after the discussion of Messrs. Vaughan and Epton's paper had been closed and replied to:—

While at some works the other day I came across last year's October number of your proceedings, and, seeing the discussion on the paper "Wire Ropes Used for Winding: Their Strength, and Some Causes of its Reduction," by Messrs. J. A. Vaughan

TABLE C.

No.	Tests in Chemical Laboratory.					Diameter and Description.	Tests in Mechanical Laboratory.			
	Strength of Sulphuric Acid.	Time of Exposure.	Loss of Iron in gms. per sq. in.	Remarks.	Conclusions drawn from Chemical Test.		Breaking Load in lb.	Percentage Elongation.	No. of Torsions in 8 in.	No. of 180° Bends over ¼in. Radius.
					IMPROVED PLOUGH STEEL.					
1	0·071% at 66° F.	49 hrs.	·0481	Plain	This acid is about five times as strong as most mine waters, and had dissolved most of the zinc coating.	·080 in. plain	1,200	3·1	38	9
			·0018	Galvanised		·083 ,, galvanised	1,220	6·9	32	7
			·0476	Plain						
			·0014	Galvanised						
2	0·025% at 70° F.	21 hrs.	·0081	Plain	Acid in thin layer just covering wires in a flat dish. Practically same results as in No. 4, therefore contact with air makes no difference.	·092 in. plain	1,580	3·4	36	9
		21 ,,	·0009	Galvanised		·095 ,, galvanised	1,620	7·5	29	7
		43 ,,	·0231	Plain						
		43 ,,	·0033	Galvanised						
					MILD PLOUGH STEEL.					
3	0·071% at 67° F.	48 hrs.	·2255	Plain (not aerated)	Aeration has therefore very little effect on the corrosion. This wire is much more easily attacked than No. 1.	·103 in. plain	2,020	4·4	31	7
			·0079	Galvanised ,,		·105 ,, galvanised	2,000	7·8	24	5
			·1972	Plain (aerated)						
			·0094	Galvanised ,,						
4	0·025% at 68° F.	43 hrs.	·0097	Plain	Zinc apparently unaffected.	·092 in. plain	1,560	3·7	37	8
		43 ,,	·0011	Galvanised		·096 ,, galvanised	1,640	7·5	29	6
		66 ,,	·0231	Plain						
		66 ,,	·0067	Galvanised						
5	0·071% at 66° F. with ferrous sulphate added.	48 hrs.	Not measured, owing to added iron.		A much more marked corrosion resulted in the unprotected wires, but the galvanised ones were little attacked, although they lost most of their zinc.	·080 in. plain	1,160	3·4	40	11
						·083 ,, galvanised	1,180	7·5	32	8
					SPECIAL IMPROVED PATENT CRUCIBLE STEEL.					
6	0·025% at 70° F.	21 hrs.	·0108	Plain	This wire is more easily corroded than Nos. 1 or 4.	·103 in. plain	1,540	3·7	30	7
		21 ,,	·0009	Galvanised		·105 ,, galvanised	1,660	7·5	24	6
		44 ,,	·0151	Plain						
		44 ,,	·0021	Galvanised						

and W. Martin Epton, I got the loan of it, it being a subject I am much interested in.

I do not know if you care to receive any comments from engineers other than members, but in case you do, I have pleasure in submitting the following remarks, hoping they may be considered of value.

With regard to Mr. Williams' remark that ropes intermittently wetted with a weak solution (·08 per cent.) of H_2SO_4 gradually become coated with a strong or concentrated solution owing to the evaporation of the water, he will, I am sure, find that this result does not take place, as the water and acid are so intimately mixed that they cannot be separated by ordinary evaporation, while the intermittent wetting with the weak solution will restore and maintain the acid solution on the ropes at the original density.

On the question of galvanising, there is no doubt whatever that it has an injurious effect on steel wire, and, consequently, on wire ropes, the ill effects being greater as the quality of the steel improves and as its tensile strength increases.

The deterioration, however, is not due to the simple fact of its having a coating of zinc on it, but is owing to the necessary HCl cleansing process preparatory to the galvanising, while the heat of the bath reduces the temper of the steel, and which cannot be subsequently recovered by a further drawing after the wire has been galvanised.

Owing to the steel being ductile, while zinc practically is not, whenever a coated wire is subjected to a twist test the galvanising always cracks and peels off, and any roughness of the coating cannot possibly induce cracks in the steel. During my practice for some 25 years as a consulting and supervising engineer I have had a considerable number of wire ropes through my hands, and only recently have had occasion to make a large number of tests on steel wire for use in the Transvaal, and these, as well as previous experiments, clearly show that galvanising both reduces the tensile strength and the number of twists obtainable in a given length.

From an examination of a large number of tests I have carried out it appears that the average tensile strength of bright wire was reduced ·3 per cent. after galvanising (although in a few isolated cases I have had an increase of 5·3 per cent., but this is probably due to inaccuracy, due to rapid testing in the works), while the number of twists in 6 in. was reduced 34 per cent. The samples of wires were, in each case, taken from the same coils, and within a few feet of each other, just before and after passing through the galvanising bath; the quality of the steel was 76 tons per sq. in., ten twists in 6 in., and had 0·7 per cent. of carbon.

It has been inferred from Dr. Moir's experiments that galvanising improves the wire, and the proof of this is that the elongation was practically doubled (it must have been a very hard wire originally). This, however, I think, will be found on further investigation as not an improvement, as I have no doubt the wire tested was a high tensile and moderate extension one, and that the effect of the heat of the galvanising bath lowered its temper, gave it a higher elongation, but a somewhat lower tensile strength, and much less twisting resistance, which characteristics were clearly not required by the specification to which the wire was drawn.

It may be argued that, inasmuch as the number of twists is reduced by the process of galvanising, the elongation should also be reduced. The effect, however, of the acid bath is only a surface one, and is not felt in the wire when under a direct pull (unless there was a fault in the wire and allowed the HCl to get in), but only when the surface is subject to twisting or sliding action on its neighbouring particles, the effect being greater as the diameter increases.

With respect to drawing of wire after being galvanised, this cannot be effected for the reasons previously stated, viz., that zinc has no practical ductility.

The effect of the increased cross section of wire, due to the coating of zinc, is of no practical moment. Firstly, the increased area due to ordinary galvanising is infinitesimal, and while the tensile strength of the wire may be 65 to 90, or even 110 tons per sq. in., zinc is only about 3 tons at most. As an example, some wires I tested had a diameter of ·160 in. when bright, and ·1625 in. when galvanised, giving an increased area of ·00063 sq. in., with a corresponding increase of resistance, due to the zinc, of only 4·23 lb. as compared with 3,900 lb., the breaking strength of the bright wire.

As a protection for wire ropes, no doubt, galvanising is frequently advisable, but it is

always a question of balance of advantages. If this be done, then the total strength of the rope should be made slightly more than otherwise required, to allow for the deterioration effects previously described, and then they should be further and constantly protected by grease, free from all traces of acid, and so prevent galvanic action in case of any abrasion taking place in the zinc surface.

With reference to the ultimate strength, as given in a testing machine, of a round rope as a whole, I put this as about 75 to 80 per cent. of the sum of the strengths of the individual wires.

With reference to the testing of wire ropes to destruction, it is most difficult to obtain anything like the full efficiency of the sum of the individual wires, chiefly because of the difficulty in getting a good grip of each wire. If, however, the grip could be made long enough to allow every wire of every strand to come to the surface, then a very close approximation would be obtained.

In an ordinary testing machine the best results I consider possible in a compound rope would give an average of 87½ per cent. in a 6/7 strand rope, each strand having 6/7 wires, and about 75 per cent. in a 6/7 strand rope with 19 wires and upwards in each.

The ropes for the Sydney Middle Harbour Suspension Bridge, N.S.W., which I was responsible for, had a circumference of 7¼ in., 7 strands, each of 27 wires, diameter of wire ·1225 in. The breaking strength of the wire was 110 tons per sq. in., and giving 16 twists in 6 in. The total value of the 189 wires was 210 tons. This rope, when tested as a whole to destruction by Mr. David Kirkaldy, gave a percentage of efficiency of 76·1 per cent., reckoned on the specific strength of the wires, but 67 per cent. if reckoned on their actual resistance obtained by testing the wires singly.

Extract from "The Journal of the Transvaal Institute of Mechanical Engineers," Vol. IV., No. 8, 1906:—

SOME INTERESTING REPAIRS TO MACHINERY.

By Mr. K. Schweder.

As a third example, I would like to show you a sketch (Fig. 5) of a cast-iron hoisting drum in cross-section, which some time back developed cracks through the flanges joining the two halves of the drum together.

We first stayed these flanges by [irons, but without success. We then unwound the winding rope and spliced into the end of it a piece of old rope, sufficiently long to form one layer between the flanges of the drum.

We fixed the free end of the old rope to the drum in the usual way, and wound it on under a very moderate strain. This layer always remains unwound, and as a few rounds of the second layer are likewise kept on the drum, thus covering up the splice at *x* and locking it, we are complying with the necessary regulations.

By adopting this method we saved the expense of new rope for forming the bottom layer, which might be called dead, as it is never used for winding. A similar saving will be effected each time a new rope is required.

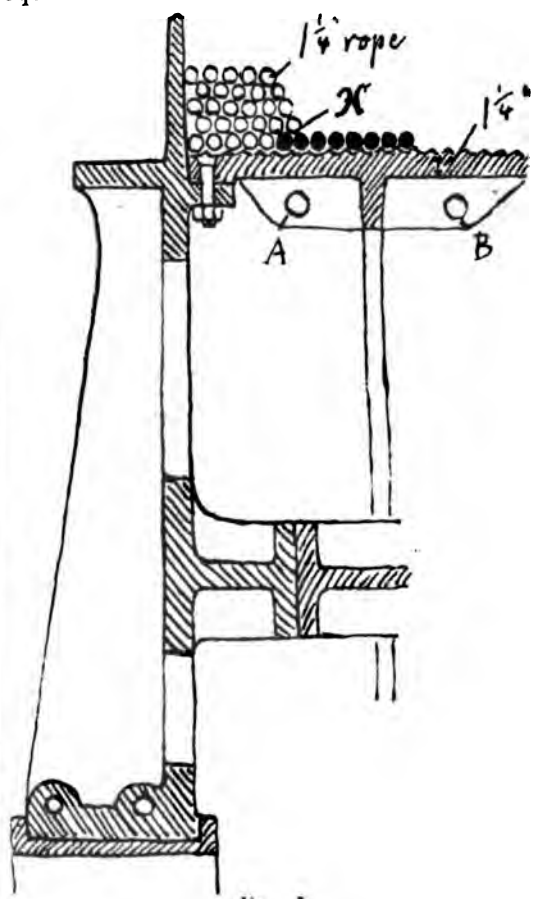


Fig. 5.

There has been no further extension of the cracks since we made this arrangement some eighteen months ago, and it might be interesting to consider how the drum shell is relieved on account of the first layer of slack rope.

Supposing the winding strain S in the rope be uniform (which is only the case with tail ropes) and the first layer on the drum is coiled on under this strain, then the crushing strain in the drum shell per square inch $= \frac{S}{1{\cdot}25 \times 1{\cdot}25} = \frac{S}{1{\cdot}56}$ because the drum shell is 1·25 in. thick and the rope is 1·25 in. diam.

If the cross area of the rope $f = {\cdot}56$ sq. in., then the strain per square inch drum shell is, to my belief, if a second layer of rope is added, $\frac{S}{1{\cdot}56+f}$, and if a third layer of rope is put on the drum, the additional strain per square inch cross section in the drum shell is $\frac{S}{1{\cdot}56+2f}$.

The total crushing strain per square inch, due to three layers of rope which were each put on under the strain S, amounts therefore to—

$$\frac{S}{1{\cdot}56}+\frac{S}{1{\cdot}56+f}+\frac{S}{1{\cdot}56+2f}=\frac{S}{1{\cdot}56}+\frac{S}{2{\cdot}12}+\frac{S}{2{\cdot}68}$$
$$=1{\cdot}48\ S.$$

On the other hand, if the first layer on the drum be put on with moderate strain, and three layers coiled on top of it with working strain S, then the strain per square inch in the drum shell section will be only—

$$\frac{S}{1{\cdot}56+f}+\frac{S}{1{\cdot}56+2f}+\frac{S}{1{\cdot}56+3f}=$$
$$\frac{S}{2{\cdot}12}+\frac{S}{2{\cdot}68}+\frac{S}{3{\cdot}24}=1{\cdot}15\ S.$$

The specific crushing strain is thus reduced by $\frac{1{\cdot}48-1{\cdot}15}{1{\cdot}48}=22$ per cent., but the slack layer on the drum is, apart from this, forming a kind of a bridge over the division of the drum, and strengthens the flanges all the more, and the drums gave no more trouble as already stated.

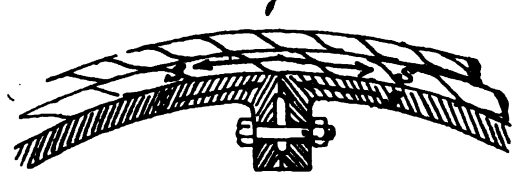
Although these examples themselves might not lead to much discussion, I thought they might be of considerable interest to members, and induce others to come forward and give their experience in similar cases, as on almost every mine certain repairs and alterations crop up from time to time, and I think many of them would be well worthy of description and of use to the resident engineer.

THE PRESIDENT (Mr. E. FARRAR): I think you will agree with me that Mr. Schweder has introduced quite a valuable new departure in our proceedings. It is not perhaps entirely new, because we had a paper from Mr. Spengel, on repairs to a crank shaft, but it certainly deals with a subject of great interest and great importance. The making of repairs sometimes requires very great care and very great thought, and occasionally involves considerable anxiety, and I think it would be well if those of you who have carried out repairs of a particularly intricate and difficult character would give the benefit of your experience to the Institute. I am not quite clear if I understood Mr. Schweder in the last drum problem. I understood him to say that the compression stress on the drum was reduced by the additional layer of rope.

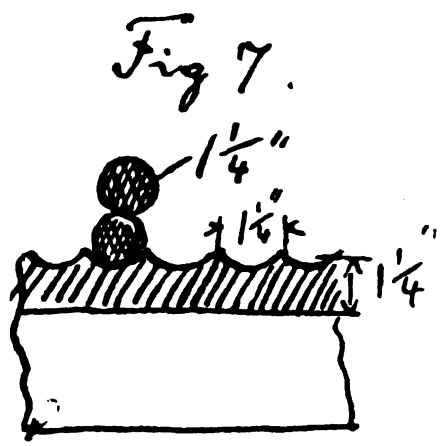
Mr. J. A. VAUGHAN: It is generally accepted on these fields that additional layers are detrimental to the life of a rope. That means to say that additional pressure is created. Whether it is so or not I would not like to state. It is the prevailing view that when there are riding turns, the under layers are not relieved of any stress at all: they rather get more. I should like Mr. Schweder clearly to explain his reasons for considering that the drum was relieved of stress by the winding of one standing layer of rope on it.

Mr. K. SCHWEDER: When I was in Europe I had a discussion with the makers of winding engines on this point, and they were not quite clear on the subject. It took me a good deal of thinking before I came to this theory that I have brought forward to-night. I am glad some discussion has started on the point, as it gives me an opportunity to explain a little more on the subject. What I mean is this: If you look at a drum shell in radial cross section (Fig. 6), and the rope being put on in the ordinary

Fig 6



way under a winding strain, S, this strain is transmitted as a crushing strain into the cast iron body, and this crushing strain is—if you agree with me—of the same magnitude as the tension in the rope. That is the first layer. The specific crushing strain—looking now at the other cross section in the cast iron drum (Fig. 7)—on this area equals S divided by the total area. The width is 1¼ in., and the thickness is 1¼ in. The square of 1¼ equals 1·56. That is the specific crushing strain on the cast iron drum. If you put a second layer on top of it, I maintain (but this is only my theory) that this first strain remains in the drum as the result of the first layer, and in addition to that we have got a second specific strain equal to S, divided by the cross area in the drum shell, plus the cross area of one rope. That is 1·56 plus F. Now, you might say that the rope cannot take any compressing strain if you have another layer on top of it, and that it would rather bulge out. As long as it is under tension or enclosed by other coils it cannot bulge out, and the cross area will assist against an additional strain. Therefore, if a third layer comes on we get plus S divided by 1·56 plus 2F. Of course the specific crushing strain



increases with each layer, but not in the proportion of the layers. It is not doubled by the second layer, but it is increasing.

Mr. J. A. Vaughan: Did you find in this particular experiment that the stress on your drum was relieved by putting that strain on? Engineers agree that the most damaged part of the winding rope is the bottom layer on the drum.

Mr. K. Schweder: The wear of this old rope might be more than the wear of the new one, but I do not know that we have anything to show to that effect. The first layer of the drum wears the most because it has to stand the immediate crushing strain of the other layers.

Mr. J. A. Vaughan: The theory of one eminent engineer on these fields is that the drum under pressure contracts, and so practically relieves the first layer of the rope of some of its tension. Does Mr. Schweder think that might occur?

Mr. K. Schweder: One must think in a certain way, and form a certain idea how these stresses are acting and re-acting, and my idea of the subject is that if one rope is put under a strain, it transmits the strain right on to the drum as re-action. If the second layer comes on to the first layer, this is the active part, and the other parts below are re-acting.

Mr. J. A. Vaughan: The point I brought up was this: If the first layer is laid on at a certain tension, any additional layers of rope wound on after that will, so to speak, put an additional compression on your drum, and so relieve the first layer of the tension which was originally put on.

Mr. K. Schweder: Certainly it does. In the case of two working layers the top rope would not relieve the bottom rope entirely of the inert tension, because the top rope tension is transmitted to the bottom layer and to the drum as well. The re-action is divided between the drum and the first layer of the rope. Supposing we had no drum but only the bottom rope, then the second layer would take the tension out of this rope entirely.

Mr. W. Martin Epton: It applies to flat ropes just the same?

Mr. K. Schweder: Yes. I come now again to the point how the lowest rope really relieved the drum. If you have one layer without any tension, then the first working layer of the rope transmits the tension to two re-acting cross areas. Therefore you start with the formula that the total specific crushing strain equals S divided by 1·56 plus F. For the next layer the strain equals S divided by 1·56 plus 2F, and for the third layer it equals S divided by 1·56 plus 3F. The result of that equals 1·15 S. Therefore, we have reduced by the standing layer the specific crushing strain from 1·48 S to 1·15 S.

Mr. J. A. VAUGHAN: Your point is that the tension does not go further than the point of application. If the rope gets any release owing to the drum shrinking under the pressure, there is no sliding of the layers one along the other, on account of the fact that the winding tension is not transmitted far enough along the previously wound on portion of the rope.

Mr. K. SCHWEDER: Oh, no.

Mr. J. A. VAUGHAN: You think it goes right through?

Mr. K. SCHWEDER: Naturally. It changes. The tension of one round is come down to a half.

Mr. J. A. VAUGHAN: It is diminishing.

Mr. K. SCHWEDER: Yes, diminishing. Perhaps you could form a better idea if you had to deal with an elastic belt—an elastic belt that is rather stretched. If you put another rubber belt on under tension—another layer—this layer would take out the tension and would even, if possible, convert the tension into compression.

Mr. J. A. VAUGHAN: The chief point you have brought forward is that you found in this particular instance that the stress on the drum was relieved by the application of a standing layer. If this is so, I think you have proved, practically, a most important point. I understand that, after laying a standing layer on the drum, you found these cracks did not increase in size?

Mr. K. SCHWEDER: Yes. We first tried to stay the crack by channel iron stays, but this did not prevent further cracks appearing. Then we put on the rope, and, as I have already stated, the compression in the cast iron drum was reduced by 22 per cent., as I believe besides that, the rope formed a kind of protection, because the division flange faces being not in perfect metallic conjunction, that is they slipped when the tension came on the rope, and this gave a very bad bending movement.

THE PRESIDENT: It appears to me that the initial tension of that bottom rope still remains there. The equation as to the stress is quite right if you eliminate the F. In gun making they go in for wire winding on the theory that the tension is not on the inner coils. I cannot conceive how that rope can come into a state of compression, and I quite fail to see how it can take in the stress.

Mr. J. A. VAUGHAN: Winding engine drivers tell me that when ropes are well greased they will lay up a great deal closer than when they have got no lubrication. In a case like that I consider that the tension has run through almost to the connection point. The question is, when we lay an extra layer on the drum, in so doing, do we discontinue the tension through the rope or not? If we discontinue the tension, I can quite understand that it relieves the drum of strain, but if the tension is continuous throughout that rope from start to finish, then by adding these layers we increase the strain. I think it is a very practical question, and Mr. Schweder is much to be thanked for bringing up this instance. I am sorry, however, that I cannot follow his explanation.

Mr. L. C. BAYLES: If the drum is going at a high speed with practically no rope on it, the drum would tend to burst by centrifugal force. If you wind a coil of rope on the drum you relieve the flange of all further strain, and if that coil of rope is left on the drum you would be perfectly safe in taking out the bolts from the flange.

Mr. K. SCHWEDER: Mr. Bayles seems to imply that centrifugal force was really the cause of the crack. This is a point I cannot agree with at all. If that was the case the cracks must be on the outside of the drum and not on the flanges. The centrifugal power of a drum going not more than 60 revolutions per minute is not sufficient to produce the cracks. Replying to Mr. Vaughan, I do not maintain that an additional layer of winding rope will strengthen the drum, but if an additional layer is put on, on top of the bare drum under no tension, this will help the drum. This layer is simply nothing more than an increase in thickness of the cast iron drum shell. In fact, when these cracks occurred, my first intention was to put something round the drum—flat iron, or another shell, or something of that sort—and then it struck me it would be much easier to put round a coil of rope, which would help the cast iron drum shell to bear the crushing strain on it.

Mr. J. A. VAUGHAN: It is just according to how you lay it on. If you lay it on with perfect lubrication the tension would be transmitted right through, and you would get not a dead layer but a live layer.

Mr. K. SCHWEDER: I am quite convinced that the working tension of the rope cannot

go further than two or three rounds of the rope.

Mr. J. A. VAUGHAN: I noticed that an engineer writing recently alluded to the point of the standing turns of rope on the drum, and stated that three or four turns on the drum were really not sufficient to run the whole tension out, but that the connection with the drum finally is the thing that the engine driver should depend upon. He pointed out this fact, that very often when a rope is put on. at the connection with the drum, and in the first layer, the rope may be quite slack, but after a week's work this is all pulled quite tight; that is to say, the tension goes right through.

Mr. H. H. JOHNSON: I think myself that if Mr. Schweder had carried his original plan into effect he would have had better results. That is simply putting a dummy shell on to the drum, because his explanation of the coils of rope forming a bridge over the joint is probably the reason why the cracks did not extend any further.

Mr. K. SCHWEDER: I quite admit that it was my intention originally to put something else on, but this was the only hoisting engine on the property, and if we had put on a dummy shell it would have interfered with the work considerably, quite apart from the cost. The old rope cost nothing, and there was practically no interference with the usual work of the mine.

THE PRESIDENT: My objection to Mr. Schweder's theory is that I cannot conceive of a rope taking the compressive stress which it has to do if it is going to relieve the drum.

Mr. K. SCHWEDER: The standing layer under the working rope gets the compression. In the working rope only the tension is relieved by the consecutive tension of the following layer, and it is reduced but not taken out entirely. In order to cause a compression, you first have to take out the tension of the layer below. Of course, the compression is not so effective as the reduction in tension for the rope, I quite agree with that.

Mr. J. A. VAUGHAN: I do not think this discussion should be closed without a reminder that these longitudinal webs on a drum are of no value whatever. Where you have to join the drum up in semi-circular halves these webs must be used, but they do not add strength. If you want to strengthen your drum at all you should strengthen it with transverse webs, for longitudinal webs on a drum are a source of weakness and not of strength.

THE PRESIDENT: I know of an instance where a winding drum has been winding for a short depth for a long time without any wear at all. When the depth was increased, and there was more weight on the drum, it began to wear.

Mr. W. MARTIN EPTON: When a drum begins to fail, owing to winding from greater depth, it is not always due to the compression action but to the wedging action of the ropes themselves.

Mr. J. A. VAUGHAN: In our experience where hoists have been used for greater depths than they were designed, the wedging action between the rope and the flanges usually breaks them down. That is really how the geared hoist breaks down at great depth, the greater number of layers, the greater leverage exerted against the root of the flange.

Mr. K. SCHWEDER: Regarding the point the Chairman brought up I think I am not quite understood, and I should like to say again according to my theory each consecutive layer puts down an additional crushing stress into the drum shell, but this additional stress is not so much as the working layer below had produced. If we have a specific crushing strain of 4 tons per square inch in the drum shell with one layer on. we do not have 8 tons with the second layer on, or 16 with the third, but they decrease in a certain manner, and this reduction of the additional strain I tried to work out on the blackboard.

THE PRESIDENT: With that diminishing layer you get to a point where there is no tension at all!

Mr. Schweder replied by a diagram.

The meeting then terminated.

SOME INTERESTING REPAIRS TO MACHINERY.

FURTHER CONTRIBUTIONS.

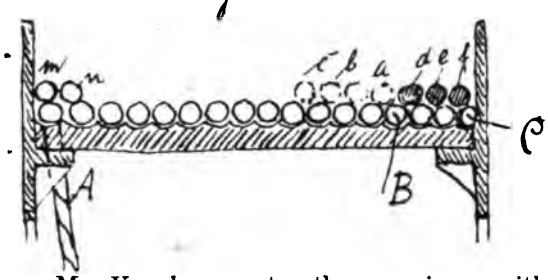
Mr. SCHWEDER: Referring to Fig. 8, I understand Mr. Vaughan's question, whether the tension brought on by the working coils *a*, *b*, etc., is not transmitted to the

very end of the rope at A. I do believe that the standing coils *d*, *e*, *f* have a kind of a locking effect, and the coiling of the layer *a*, *b*, *c*, etc., to the left has, if anything, the effect of slackening the rope towards the end A.

When the layer unwinds from *m*, *n* towards the right, the slack in the rope A to B is likely to equalise again, while I cannot fancy that any alteration will take place in the rope piece B to C, which is locked under *d*, *e*, *f*.

If it were not so, and the working tension were running through to the point A, then the additional (standing) layer of rope would rather weaken instead of strengthen the drum, which is not shown by the practical experience.

Fig 8



Mr. Vaughan quotes the experience with flat ropes on reels where the rope slips over itself and the tension goes down to the very end fastening, but this is different from the round rope proposition where a kind of self working between B and C is surely happening. If Mr. Epton's question before referred to the same flat rope on the reel, this reply stands for it as well.

In conclusion, I quite admit that the portion of the reacting compression taken by the underlying rope, and that taken by the drum shell, might be dispensed with, and it is more accurate to take it in proportion to the reversed product of area and modulus of elasticity. But who is determining the modulus of elasticity of a hoisting rope under compression when encircled?

It is, therefore, simpler and nearly as accurate, I believe, to eliminate the modulus of elasticity for the formula, and take the results from it only as approximations as what they were meant to be. If one of the members can advance a theory which is more lucid to me. I will be quite pleased to adopt it. In any case, we want in this country something in the shape of a formula to determine the thickness of drum shells, where we are using almost at every mine one or more hoisting engines with overlapping ropes.

Mr. A. Ostreicher: The armed drum problem is a very interesting one, as it contains a number of conditions which make a correct investigation so complex that even in the hands of a mathematician many influences have to be neglected, so that the result would be, even in the best case, an approximation. I therefore, after trying to solve the problem by means of calculus, attempted to surmount the difficulties in the following manner, which does not claim correctness, but is easily understood.

If a rope is coiled over the drum with a very small stress, but in such a manner that the arming ropes fill the space between the hoisting drum and hoisting rope tightly, then they are, by the hoisting rope, which has a constant tension, compressed. They change their cross sections, and their original diameters diminish. If the ropes are packed tightly, then the deformations of the cross sections may be neglected as they are comparatively small, and only the deformations in the circumference considered. The assumption of the constancy of the cross sections of the supporting parts, that is, of arming ropes and drum, causes certain errors, but as ropes and the cast iron drum do not follow Hooke's law, on which, up to the present, all statical investigations depend, I doubt that by means of the introduction of an arbitrary Poisson's ratio, something better could be obtained than a rough approximation.

Now to my method of investigation.

If the hoisting rope, which is thought to be fastened to the drum, is wound round the armed drum, each element is subjected to the constant tension T. If we imagine the hoisting rope cut in a diameter, then equilibrium can only be maintained when, at certain points of the two cross sections, external forces are added which are equal to the tension of the rope T. The resultant of the added external forces 2T will pass through the centre of the drum, as the external forces lie symmetrically, and 2T is also the resultant of the internal forces.

Now, by coiling the hoisting rope round the armed drum, the diameter of the first coil will be diminished by δD^1, and this causes an average stress.

$$f^1=\frac{\delta D^1}{D^1}E^1$$

The stress in any of the arming coils is

$$f_n=\frac{\delta D_n}{D_n}E^1$$

and in the cast iron drum

$$f=\frac{\delta D}{D}E$$

where E^1 and E are Young's moduli of elasticity for the ropes and cast iron. I mentioned that both are functions of the stress.

If δD^1, δD^{11} ... δD and E^1, E were known, it would not be difficult to determine the loads carried by the arming ropes and the drum element under the ropes.

As $P^1=A^1f^1$, $P^{11}=A^1f^{11}$ ... $P=Af$

and $2T=2P^1+2P^{11}+\ldots+2P$

or $T=A^1f^1+A^1f^{11}+A^1f^{111}+\ldots+Af$

where A^1 represents the sectional area of the arming rope, A the sectional arc of the drum part, having as width the diameter of the rope.

Assuming constancy of cross sections, then this has, as a consequence, equality of deflections, that is,

$$\delta D^1=\delta D^{11}=\delta D^{111}=\ldots=\delta D$$

or, by introducing the values found,

$$T=A^1E^1\left(\frac{\delta D}{D^1}+\frac{\delta D}{D^{11}}+\ldots\right)+AE\frac{\delta D}{D}$$

$$=AE\,.\,\delta D\left[\frac{1}{D}+\frac{A^1E^1}{AE}\left(\frac{1}{D^1}+\frac{1}{D^{11}}+\ldots\right)\right]$$

Now,

$$\delta D=\frac{fD}{E}$$

or

$$T=AfD\left[\frac{1}{D}+\frac{A^1E^1}{AE}\left(\frac{1}{D^1}+\frac{1}{D^{11}}+\ldots\right)\right]$$

$$=Af\left[1+\frac{A^1E^1}{AE}\left(\frac{D}{D^1}+\frac{D}{D^{11}}+\ldots\right)\right]$$

and the stress of the c.i. drum

$$f_n=\frac{T}{A\left[1+\frac{A^1E^1}{AE}\left(\frac{D}{D^1}+\frac{D}{D^{11}}+\ldots\right)\right]}$$

when armed, and $f=\frac{T}{A}$ when not armed.

As

$$1+\frac{A^1E^1}{AE}\left(\frac{D}{D^1}+\frac{D}{D^{11}}+\ldots\right)>1$$

it follows that the stress of a drum armed with wire ropes is smaller than the stress of the drum without arming ropes.

If the difference of the diameters are small, we get

$$f=\frac{T}{A\left[1+\frac{A^1E^1}{AE}n\right]}$$

which expresses in the symbolic language of mathematics, approximately, Mr. Schweder's statement.

Should the arming rope be wound up with a stress smaller than the stress of the hoisting rope, then the effect will be a reduction of the stress f^1, f^{11} ... whereas will be greater than f_a. It is therefore advisable to wind the arming rope with as small a stress as possible.

I know too well that the foregoing investigation gives a distorted picture of the stresses, but I doubt if by a more exact formula much would be gained, as long as the behaviour of ropes is not established by a great number of experiments, the more as it is cheaper to make a stronger drum than to arm it, and every repairing job is a matter of compromise.

How many coils can be wound round a weak drum does not depend on the probable stresses or deformations, but on conditions over which the engineer has no control.

SOME INTERESTING REPAIRS TO MACHINERY.

By Mr. K. Schweder.

DISCUSSION.

Mr. R. Price: Mr. Schweder's valuable communication has raised once more a question of vital importance, more especially to those interested in the design and construction of drums for winding engines. The theory advanced by Mr. Schweder is a fascinating one, but the writer fails at present to see that it can be substantiated.

It seems inconceivable that the arming coils can to any appreciable extent sustain a compressive load in a circumferential direction, because the rope cannot be considered to have anything like the cohesion between its component parts which exists between the particles of, for instance, the cast iron drum shell beneath it, and that, therefore, buckling of the wires will probably occur under comparatively small loads. The arming coils do, no doubt, in the circumstances, viz., when wound on under very

slight tension, sustain some of the circumferential compressive stress. In the opinion of the writer, however, this amount is very small, and the greater part of the tensile strain in each successive coil is transmitted as a simple radial compressive strain to the drum shell.

The writer notes that the rope which was used for the arming coils was an old one, a possibly favourable circumstance, because its modulus of elasticity would probably closely approach, if it did not actually exceed, that of the drum material.

This refers, of course, to the modulus of elasticity under tensile stress. In the absence of any figures relating to the behaviour of wire rope, and its modulus of elasticity when subjected to compressive stress, the writer prefers to consider the question at least an open one.

Mr. Schweder, for the purpose of rough calculation, assumes that the load S on the rope is constant, although he points out that this is the case only where tail ropes are used.

In view of the fact, however, that the weight of the rope in deep shafts is frequently equal to, or even greater than, that of the suspended load, the writer would be indebted to Mr. Schweder if he could furnish further particulars as to the load carried by the rope, the depth of the shaft, the diameter and width of the drum, the general section of the rim and its attachment to the arms and flanges, and the weight of the rope.

The question is so complex that it seems almost impossible to arrive at a general formula which shall express clearly the effect of coiling successive layers of rope on a drum, but by taking specific cases, it might be possible to arrive at some tangible and valuable results.

Take the case of a drum winding a rock load of 8,000 lb. with a skip weighing 5,000 lb., a total of 13,000 lb., from a depth of 4,000 feet.

Assume a rope made of steel, with an ultimate strength of 268,800 lb., and having a static factor of safety of seven.

The weight of the required rope would be about 3·1 lb. per ft., and the total weight of the rope suspended 12,400 lb., or nearly equal to the external load which it carries.

In the case of a rope made of 220,000 lb. steel with a static factor of safety of seven, the weight of the rope would be about 1·4 times that of the load it carries. In the case of the first rope above cited, with a drum, say, 10 ft. diam. x 4 ft. wide, the number of layers would be nearly four, and the decrement of weight per layer about 3,500 lb., so that in such a case the cumulative strain transmitted to the drum, though great, would, after all, not amount to anything like four times the load on the first winding coil.

Returning now to the origin of the repair described by Mr. Schweder, it may be conceded, as has been shown by previous writers, that when a rope is coiled round a drum under a given strain, that strain is transmitted to, and appears in the rim of the drum, as a compressive strain of the same amount, that the diameter of the drum is reduced at each point in its width by an amount inversely proportional to its cross sectional area at that point, and that this reduction is, therefore, in normal cases greater at the centre, where the cross sectional area is less, than at the sides, where the compressive stress is supported very largely by the flanges and rims of the drum.

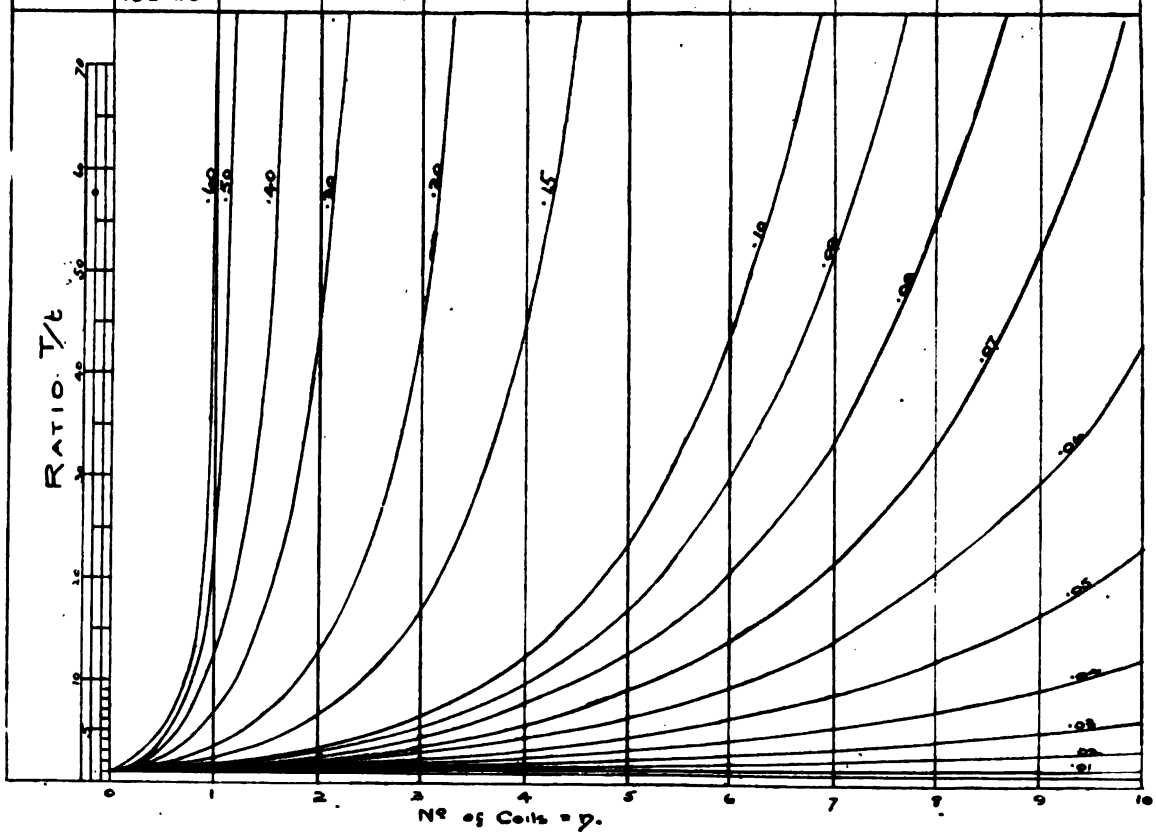
Further, that the result is the bending inwards at the centre of the drum, inducing tensile stress in the inner fibres, and that transverse ribs cast inside the shell with the object of stiffening it only aggravate the evil, unless they are made very deep, because cracks nearly always supervene in these ribs, and under repeated application and removal of the load gradually extend into the body of the shell, which will eventually split circumferentially.

The above remarks are largely quoted from the paper on "Winding Plants for Great Depths," by Mr. H. C. Behr, who has also shown that "when drums have to be made in sections jointed together by flanges, these flanges should be made in the form of separate short lugs, the cross section of the drum being reduced for some distance circumferentially on each side of the joint, giving greater flexibility at these points, to compensate for the rigidity of the lugs, the necessary cross sectional area to sustain the compressive stress being obtained by thickening up the metal immediately under the lugs."

It would appear to the writer that in the case cited by Mr. Schweder, the fracture of the flange through the edge into the bolt holes automatically produced a structure

TABLE and DIAGRAM showing the varying relation between "T" the tension at the loaded end, and "t" the tension at the "slack" end of an element such as a rope wound "n" times round a drum with various coefficients of friction μ. The table is calculated from the formula $\text{Log}\frac{T}{t} = .4342944\,\mu\theta$ in which θ expressed in circular measure is $2\pi n$. - The expression $.4342944 \times 2\pi = 2.72875$ is a constant $= c$, and the formula becomes $\text{Log}\frac{T}{t} = \mu c n$.

Coeff. of Friction μ	RATIO T/t									
	$n=1$	$n=2$	$n=3$	$n=4$	$n=5$	$n=6$	$n=7$	$n=8$	$n=9$	$n=10$
.01	1.0648	1.1330	1.2074	1.2857	1.3691	1.4579	1.5527	1.6531	1.7603	1.87444
.02	1.1329	1.2857	1.4579	1.6531	1.87444	2.12544	2.4100	2.7328	3.0987	3.5136
.03	1.2074	1.4579	1.7603	2.12544	2.5663	3.0987	3.74114	4.5176	5.4546	6.5860
.04	1.2857	1.6531	2.12544	2.7328	3.5136	4.5176	5.8083	7.4679	9.6017	12.345
.05	1.3691	1.87444	2.5663	3.5136	4.8104	6.5860	9.0170	12.345	16.902	23.140
.06	1.4579	2.12544	3.0987	4.5176	6.5860	9.6017	13.998	20.408	29.752	43.376
.07	1.5527	2.4100	3.74114	5.8083	9.0170	13.998	21.731	33.737	52.373	81.306
.08	1.6531	2.7328	4.5176	7.4679	12.345	20.408	33.736	55.770	92.194	152.40
.09	1.7603	3.0987	5.4546	9.6017	16.902	29.752	52.373	92.194	162.28	285.68
.10	1.87444	3.5136	6.5860	12.345	23.140	43.376	81.306	152.40	285.68	535.49
.15	2.5663	6.5860	16.902	43.376	111.31	285.68	733.14	1881.5	4828.5	12391
.20	3.5136	12.345	43.376	152.40	535.49	1881.5	6610.8			
.30	6.5860	43.376	285.68	1881.5	12391					
.40	12.345	152.40	1881.5							
.50	23.140	535.49								
.60	43.376	1881.5								
.80	152.40									



approximating to that just described, and that the cracks would probably not have extended any further into the body of the shell.

In this connection it might be well to remember that cylindrical castings, if great care be not exercised, frequently cool more rapidly outside than inside, and in this way very high initial tensile stress may be set up in the inner fibres, so that such a drum might crack under a load which would be safely carried by a casting which had been more uniformly cooled.

Mr. J. A. Vaughan (see p. 117, March Journal) has referred to the fact "that ropes when well greased will lay up a great deal closer than when they have got no lubrication." He considers that "in a case like that the tension has run through almost to the connecting point." The writer has prepared for his own information (and possibly it may be new in that form to other members) a table and rough diagram which show the relation between the strains at the loaded and the slack ends of a tension element such as a rope wound round a drum n times, with varying co-efficients of friction μ.

The table is merely an amplification of existing tables relating more especially to tension elements, such as belts or ropes on pulleys, but in which n is usually taken less than unity, and the co-efficients of friction assumed are not so numerous.

Taking the case of the regulation number of three coils, with a co-efficient of friction of ·10, it will be seen that a load of 6·58 units at the end of the third coil will induce a resistance of 3·51 units at the end of the second coil, 1·87 units at the end of the first coil, and 1 unit at the point of connection. Or, conversely, taking this time the co-efficient ·15, a force of 1 unit at the point of connection will sustain 16·9 units at the end of the third coil, 28·5 units at the end of the sixth coil, and 12,391 units at the end of the tenth coil.

Take again the case of the co-efficient of friction ·1. A unit force at the point of connection will support a load of 1·87 units at the end of the first coil, whereas if the co-efficient were ·01, ten coils would be required to support the same load, and at the end of the third coil T would be only 1·20 t This bears out to some extent Mr. Vaughan's statement, but the co-efficient ·01 is probably far lower than anything that could exist between a wire rope and a drum.

Suppose now that a total load T be suspended from a drum on which there are, say, the three regulation coils of rope.

Assume μ as before = ·10.

Let n = number of coils is one layer, and w = weight of one coil of rope.

The tension t at the point of connection is $\frac{T}{6\cdot58}$. The tension at the end of the fourth coil will be $(T - w)$ and at the end of the fifth and succeeding coils $(T-2w)$.....$T-w(n-3)$.

The question arises as to whether the relation $\log \frac{T}{t} = \mu c n$ is true only of the first three coils, which never leave the drum, or whether it also holds good for the whole of the coils in the first layer. If so, it would mean that the tension at the end of each coil is somewhat relieved owing to the gradually decreasing tension in each successive coil wound on.

It seems possible that some such action may take place in each layer taken by itself, though it does not seem probable that any such release could take place as between layers, because each successive layer must tend to lock the one below it.

It would appear from the above investigation that although it may not accurately indicate what takes place, still it is inconceivable that except with a very low co-efficient of friction the tension at the point of connection, or anywhere near it, can never exceed that found by the relation $\log \frac{T}{t} = \mu c n$.

In any case, the writer must agree with Mr. Vaughan that the tension in each successive layer of coils, whatever its magnitude, is transmitted almost entirely to the drum shell, which practically has to bear the whole strain.

SOME INTERESTING REPAIRS TO MACHINERY.

By Mr. K. Schweder.

DISCUSSION.

Mr. E. J. Laschinger: Mr. Schweder's remarks on Machinery Repairs are of interest and value.

There is only one of the cases which he mentions that I would touch upon, and that is the expedient he adopted in the case of a C.I. winding drum. I gather from his description that the drum showed signs of failure by the cracking of these continuous flanges, which made the joint of the halves of the drum. These continuous flanges, in a cast iron drum especially, are a mistake and a source of weakness, as was ably pointed out by Mr. H. C. Behr in his well-known paper on "Winding Plants for Great Depths." The beam action on such flanges induces tremendous tensile stresses, in the inner edge of the flange, which develop cracks. The continual working caused by the winding on and off of the rope, deepens and spreads a crack once begun, until the whole drum-shell may be split circumferentially. Joints should, therefore, not be made with continuous flanges but by lugs only to take the necessary bolts, and but few bolts are needed. This makes the joint elastic. The writer has in mind just such a case as quoted by Mr. Schweder, where cracks developed after but a few weeks, although the drum-shell itself was over 2 in. thick. The defect was remedied by drilling holes through the flanges between the bolts, at the line where the flanges merged into the body of the shell and cutting right through the flanges down to the holes, thus artificially cracking the flanges between the bolts. No further cracks developed.

Incidentally, Mr. Schweder gives a very ingenious reason for adopting the course he took in remedying the defect in his case, by wrapping a dead coil of old rope around the drum permanently under a slight strain.

That the stress induced in the shell of a drum is not that due to the sum of the strains under which each coil of rope is wound on, is well known. Mr. Schweder's opinion is that every coil of rope covered by a succeeding coil adds the area of its solid cross-section to the resisting power of the drum. I have tested the general truth of this assumption, by taking the case of a steel plate drum-shell, which was deformed by being used for work much too heavy for its design. This drum took eight layers of rope. Figuring in the ordinary way, that each coil adds a compressive stress proportionate to the strain under which it was wound on, the resultant stress became so great that the drum must have collapsed entirely; but it did not, it was simply strained beyond its elastic limit. Figuring out the stress on Mr. Schweder's assumption, the maximum stress was well within the ultimate strength of the material, but exceeded the elastic limit considerably. It is quite safe to say, therefore, that the assumption must be very near the truth.

During the discussion on Mr. Schweder's paper I have noticed that several contributors have questioned the ability of the inner coils of rope to withstand compression. It is well known, however, that if a layer of rope on the drum of an engine is not unwound for some time, it shows warts and lateral spreading of the wires, which indicate that the rope has been under severe compression. It is the usual practice to unwind such a rope quite frequently, and rewind it again under heavy tension, to prevent the rope from being ruined. If a rope is wound on tight and covered by succeeding layers, it is certainly safe to assume that the rope may maintain a heavy compression, since it cannot buckle or move out of place.

However, it is not even necessary to assume that the rope in the under coils is subjected to compression, in order to show that the stress in the drum shell is not a sum of the strains under which the rope is wound on. If a rope is laid on under a certain tension, it is stretched. Now assume the coil of this stretched rope wound round the drum. As more coils are wound on the drum, the drum shell, being also elastic, will shrink, the former stretched coil of rope will contract also, and the tension partly released, depending upon the amount of contraction of the drum and the elasticity of the rope. It is, therefore, evident that the real stress in the drum shell is dependent to a very great extent upon the modulus of elasticity of the rope and the elasticity of the shell. The writer referred to this subject in his contribution to the discussion on "Winding Plants for Great Depths."*

The bad feature in remedying the defect, according to the method adopted by Mr. Schweder, is the danger of cracking off the cheeks of the drum, by adding an extra coil and increasing the wedging action of successives layers of rope. Examples of this kind

* Transactions of the Institution of Mining and Metallurgy, Vol. XI, 1901-2, p. 277.

of failure are quite numerous on the Rand. As the depths of shafts increase, especially in such cases where the winder hoists, in one lift from the incline and up the vertical, more and more layers of rope are required on the drum, and finally the cheeks split off. This action of reversing the travel of the rope across the drum is severe both on the drum and on the rope, and also causes bad winding of the rope and nasty jerks. If a rope is to be wound on a drum in successive layers, the inner sides of the drum flanges should be so designed that the reversal is accomplished without shock; for each layer a groove should be cut out of the metal of the cheek, which will gradually raise the rope up the level of the succeeding coil, to prevent wedging and also guide the rope in the opposite direction. This may easily be accomplished by attention to design.

Mr. H. H. JOHNSON: I should like to ask Mr. Laschinger whether he can account for the difference which is noticeable between round ropes and flat ropes with reference to the warts caused by compression. As far as my experience goes I have never noticed any signs of compression on flat ropes, although one would expect that they would be subjected to greater compression than round ropes. On round ropes I have noticed, especially on sinking engines, where a considerable amount of rope may be left on the drum which never comes off, we have had to take ropes off owing to the warts interfering with the coils above.

Mr. E. J. LASCHINGER: I have never had any experience with the running of flat ropes, but I have always thought that one of the causes of failure was this: they are generally laid up in reels with open sides, and the compression which the lower coils have to stand displaces the wires and loosens up the strands of the rope. You will notice that in a flat rope at the lower end near the engine, if the rope has had much use, the strands are very much opened out, and we know very well that that gives access to acid water, which destroys flat ropes very rapidly.

Mr. H. H. JOHNSON: That would be noticeable in the portion of the rope which comes off the reel, but in the portion left on the reel I do not think you find the stitching broken or the strands opening up in the way you describe.

Mr. A. OSTREICHER: Any one can make experiments for himself with a pencil and a piece of narrow paper. If you turn the pencil the paper gets tighter. That explains why a flat rope is not so much compressed as a round rope. A round rope must be compressed. If you wind in the first instance a number of layers round a drum without any tension, and then later another with tension, then the whole gets compressed, there is no help for it, because the top layer is extended, and that causes, as I have once shown in a contribution of mine, that the bottom layer must be compressed.

THE PRESIDENT (Mr. E. FARRAR): Mr. Laschinger's method of drilling holes in the internal flange of the drum somewhat confirms my own opinion that the crack having got as far as the hole, would have stopped at the hole, and there might have been no further spreading.

Mr. E. J. LASCHINGER: We had a practical illustration on this point. The flanges of the drum shell on a winder cracked; the crack spread and entered the drum shell itself, and the whole shell would soon have been split into two parts circumferentially. This drum was repaired by bolting on steel plates and "I" beams inside the shell, bolt heads being countersunk on the outer face of the drum. This drum was described by Mr. H. C. Behr* in his reply to discussion on "Winding Plants for Great Depths." Afterwards, on the same mine, another drum of another winder began to crack in the same way, and this was remedied before the cracks had extended into the shell, as described in my contribution this evening.

THE PRESIDENT: Was this crack in the bolt hole or in the solid flange.

Mr. E. J. LASCHINGER: In the flange.

THE PRESIDENT: I think if the crack had been at the bolt hole it would probably have stopped there.

Mr. E. J. LASCHINGER: Well, that also happened, and we thought it would not go further than the bolt hole, and it did not.

SOME INTERESTING REPAIRS TO MACHINERY.

Paper by Mr. K. SCHWEDER (*Member*).

REPLY TO DISCUSSION.

In my paper, on "Some Interesting Repairs to Machinery," I expressed the opin-

*Transactions of the Institution of Mining and Metallurgy, Vol. XI., 1901-2, p. 358.

ion that it would not lead to much discussion, and I was rather pleasantly disappointed that the reverse took place, anyhow, with respect to the repaired drum.

The lively discussion on it, showed clearly that it was an interesting repair, and that the members would really like to come to a proper understanding with regard to power enacted by more than one layer of ropes in a winding drum.

My final reply need only be brief, mainly for the reason that I believe an indisputable rule for calculating the strength of a drum-shell cannot be brought forward, as assumptions have always to be made, and yet, I think a simple rule is the best for practical use, if it is fairly proved by practical examples to be an approximation to final correctness.

Now, I am very pleased that Mr. Ostreicher, and Mr. Laschinger, are on my side in this respect, and that the latter gentleman even supports the simple rule by a practical "trial calculation."

I also took the precaution, and worked out by the stresses and sizes of an old steel plate winding drum whether my formula fitted the case, and having seen this, I only made it public in this paper before us.

* * * * * * *

That the cracks in the flanges of the drum happened at all is, of course, due to faulty design, but these drums were made in 1897, *i.e.*, long before Mr. Behr's excellent paper on winding plants from great depths was read, otherwise, the flanges would have been made as Mr. Behr had suggested.

The cracks were, in the present case, stopped by the bolt holes. In other cases they might, perhaps, be stopped by drilled holes, of you can get at the place without dismantling the drum, which would mean a considerable stoppage of the work, and then you must be sure to drill at the very end of the crack.

A strengthening of the drum shell itself is, in any case, more satisfactory. The repaired drum has now worked for 2½ years altogether, hoisting not less than about 3,500 manskip, and 4,000 oreskip every month, from a vertical depth of about 1,700 feet.

Noteworthy are Mr. Laschinger's remarks in respect to the danger of breaking the flanges if too many layers of rope are put on the drum. If the latter, however, is of a fair width, so that the deviation from the headgear pulley tends to pull the rope off from the flanges, then the danger seems to me to be somewhat smaller.

I hope other members of this Association will bring forward examples of repairs of similar nature.

"SOME NOTES ON AN ELECTRICAL APPARATUS FOR ASCERTAINING THE CROSS-SECTIONAL AREA OF WIRE ROPES."

Paper by C. McCann.

In introducing to your notice this evening a new method of ascertaining the cross-sectional area of a wire rope, I do not intend to take up a lot of your time in the discussion of technical details, but to briefly outline the principles of this invention.

This invention was suggested by a recent regrettable accident occasioned by the breakage of a hauling rope on a local mine, when a popular member of this Institute was before the court. On the evidence then given all experts agreed as to the impossibility of ascertaining the cross-sectional area of a rope without measuring the size of each wire separately. It occurred to the then public prosecutor that it ought to be possible to do something electrically.

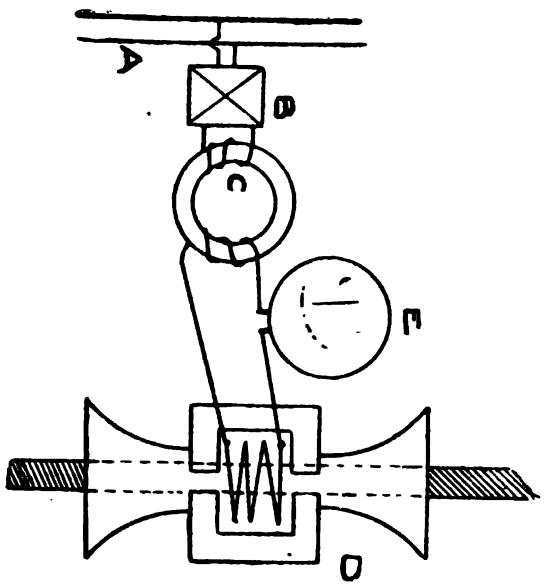
A mutual friend introduced me to him and the following is the result.

Alternating or interrupted current is taken from any convenient source, as at A, passing through the ordinary switches, etc., at B; it is taken to a transformer, C; the low tension secondary current is then applied to the coil D, one lead passing through an instrument, E, which is some form of ammeter, and may, of course, be of any size, and recording, if required. The coil is designed to open up, to enable it to be applied to a rope without the trouble of threading the rope through it. On each end of the coil proper are bell mouths; these are simply to guide the rope into the coil. The coil is also enclosed in a laminated iron circuit to concentrate the magnetic field within it.

When current is applied to the coil and there is no rope inside, the self-induction is low and the current consequently large. On introducing a rope, the self-induction is

increased and the current falls exactly in proportion to size of the rope so introduced. For instance, with this particular coil a 1⅛ in. rope will bring the current down from 220 amps. to 120 amps., and for every 1 per cent. taken from the mass of this rope the current will go up 1 ampere.

In using this coil on a mine, I think the most convenient way would be to suspend it at the collar of the shaft by four springs, then calibrate the instrument by a standard piece of rope. Having done this, the coil would then be placed round the hauling rope and the rope slowly passed through it, the person in charge of the test watching the instrument.



An immediate departure from the point shown by the test piece will take place, due to elongation; even if there be no wear or corrosion, this second point would indicate the full size rope under strain, and any bad places will be found by the instrument varying from that point to a greater or less extent and showing the actual decrease in cross-sectional area on the portion of the rope at that moment on the coil.

The principles involved are so well understood that any further explanation on my part would, I think, be superfluous.

I have a coil here which I hope you will inspect at the close of this meeting, and I hope at an early date to be able to show coil at work on a hauling rope on a mine.

THE PRESIDENT (Mr. H. H. JOHNSON): Mr. McCann's invention is so very simple, that there is no necessity for a very lengthy paper on it. I think I might safely say that it is an appliance which should be brought before the Commission, which is still sitting, on ropes and adjustments, and if they can recommend its adoption by the industry, I am sure we shall be saved a good deal of worry in the future.

Mr. W. DOCHARTY: I should like to ask Mr. McCann, if, in the event of one or more of the wires in a rope being broken internally, and the ends being still adjacent, would the instrument show there was a break there?

Mr. C. MCCANN: It could be made to do so.

Mr. W. MARTIN EPTON: It might interest the meeting if I tell the members that I saw experiments with Mr. McCann's instrument about six weeks ago. On that occasion 100 loose wires were inserted into the machine, and were withdrawn one at a time, when the difference was clearly registered on the ammetre. Of course, the most dangerous part of a worn rope is not where the wires are worn on the outside, because there they are easily seen, but rather in the centre of the rope when corrosion has taken place, and I think this instrument should be able to detect dangerous corrosion, for if the ammetre should register a reduction in the sectional area, and this is not accounted for by outside wear, then the rope should be opened up. I think I am safe in saying, that the Rope and Safety Catch Commission intend having a trial of this instrument made at one of the adjacent mines, when careful record will be kept of the readings of the instrument, and the corresponding wear on the rope.

With reference to Mr. Docharty's question, if the broken wires remain close together the instrument will not show the break, but if they are apart, which is usually the case, then they will show it.

Mr. KENNETH AUSTIN: May I ask if a corroded rope has actually been tested with this machine, and if so, with what result?

Mr. C. MCCANN: I have tested a rope which has been artificially corroded, and it measured the loss in metal accurately.

Mr. H. C. BEHR: I should like to ask Mr. McCann whether he has made any investigations as to the time it would take to make an examination of the whole length of the rope? Is the process continuous, or

do you stop for the purpose of making observations?

Mr. C. McCann: You can make the coil part and parcel of the headgear. It can be put near the headgear sheave, and you can have a continuous record all through. The letters patent cover not only this fundamental coil, but a device for continuously recording every trip and every variation in the rope, together with all signals given.

The President: With reference to the question Mr. Docharty raised about broken wires, I take it that there are few wires broken in ropes which are not broken on the outside and therefore visible, which the ordinary inspection will show us, and. if this instrument will show us actual loss of metal through wear and corrosion that is all we practically want.

Mr. A. Mayhew: I may say there are hardly any internal wires in ropes that are broken except through corrosion.

The President: Has Mr. Mayhew known of instances of wires breaking in a rope which are not visible?

Mr. A. Mayhew: There are no wires broken inside a rope which are visible to anybody.

The President: What I mean is, in your experience have you come across any instances where wires which were not core wires, broke inside the rope?

Mr. A. Mayhew: Inside wires are the core wires.

The President: Do you know of instances where the wires broke in the strands on the inside of the rope, where there is no actual wear from external contact?

Mr. A. Mayhew: I think not. You are asking if wires would break internally when there was no wear externally. I have never heard of a case like that. Core wires have broken on many occasions, but in my experience it has been through corrosion simply.

Mr. W. Martin Epton: In some constructions of ropes some wires never come to the surface at all.

Mr. A. Mayhew: But these are internal wires.

Mr. F. Gill: I have never known the outside strands to go first. If there is corrosion at all, it is absolutely internal. Wire ropes are things that you want to go over very carefully, but you never find corrosion on the outside strands. If there is a good engineer on the mine he stops that right away.

The President: Let me take a concrete example of an ordinary 6 strand 7 wire construction. There you may have internal corrosion. With that construction, if individual wires break, would they not break so that they could be seen from an ordinary external examination of the rope?

Mr. A. Mayhew: I certainly agree with you. I am sorry I have to disagree with Mr. Gill. I have had particular instances on these fields where wires have corroded to an extraordinary degree externally, but every engineer can easily see that external corrosion. If a rope corrodes externally, the wires certainly break eventually, but the engineer can see long before these wires break that they are going to break. With the friction over pulleys on what are commonly called turned shafts, it is very difficult for the engineer to see from one day to another whether the wires will break, but if he finds three or four wires broken near together he had better take the rope off.

Mr. W. Docharty: In furtherance of what Mr. Mayhew says, there are some ropes in which the wires never do come to the surface, as in compound construction of ropes. We had a remarkable experience some time ago with a rope, where several hundred wires were broken. When opening up the rope we found that the broken ends were practically in contact. They had not come apart at all. Will that instrument show a break of this description?

Mr. C. McCann: This particular coil would not show if the wires were in contact. If the wires were tapered out towards the break it would.

Mr. W. Docharty: I do not know that wear is the thing that we have most to be afraid of.

Mr. C. McCann: This coil will show losses of metal no matter how these are caused. If that broken wire has suffered any reduction in metal whatever, this particular coil will show it.

Mr. A. Mayhew: May I ask Mr. Docharty if he is quite sure these wires were touching in the core of the rope when he examined it? Because in all my experience I have never found internal broken

wires touching when they have been actually uncoiled from the rope.

Mr. W. Docharty: In any case, we have frequently found wires that were cracked, but not broken right through. We have got to put in a sharp-pointed instrument to pick the wire up, and after picking it up we found it had been worn halfway through.

Mr. J. A. Mills: I think it would be a very good machine to have on these Whiting hoists. I would like to ask Mr. Docharty if he found these broken wires in a Whiting hoist?

Mr. W. Docharty (in reply to Mr. Mills): I did not find them in a Whiting hoist, but in the rope attached to it.

The President: I think we might pass a very hearty vote of thanks to Mr. McCann for bringing this invention before us this evening. Mr. McCann has a model here, and it will be open for the inspection of members after the meeting.

"SOME NOTES ON AN ELECTRICAL APPARATUS FOR ASCERTAINING THE CROSS-SECTIONAL AREA OF WIRE ROPES."

Paper by Mr. C. McCann.

CONTINUED DISCUSSION.

The Chairman (Mr. W. Martin Epton): I think it is Mr. McCann's intention to have one of these instruments erected on one of the mines.

Mr. C. McCann: Yes. I think I will be able to get it done next week.

Mr. Kenneth Austin: Are we to assume from that reply that no attempts have yet been made to test any old ropes that are known to be actually reduced in sectional area?

Mr. C. McCann: A test has been made of a piece of old rope that has been discarded, but it has never been tested on the mines.

The Chairman: I suppose that after you have had it running at the Robinson Deep you would like the members of the Society to see it?

Mr. C. McCann: Yes.

"SOME NOTES ON AN ELECTRICAL APPARATUS FOR ASCERTAINING THE CROSS-SECTIONAL AREA OF WIRE ROPES."

Paper by Mr. C. McCann.

CONTINUED DISCUSSION.

Mr. J. H. Hughes: In Mr. McCann's paper, entitled "Some Notes on an Electrical Apparatus for Ascertaining the Cross-Sectional Area of Wire Ropes," we find the following statement: "On introducing the rope, the self-induction is increased, and the current falls exactly in proportion to size of the rope so introduced." Without any technical reasoning in the paper, it is difficult to understand how Mr. McCann arrives at that result. As the self-induction is dependent upon the magnetic properties of the steel wire in the core of the solenoid, these latter need only be examined.

Firstly, we have the magnetic permeability curve of hard drawn steel wire as given by S. P. Thomson in his book, "Dynamo Electric Machinery," formed by plotting the magnetising forces applied as abscissæ; and the corresponding magnetic induction in lines per square centimetre as ordinates. The result obtained is not a straight but a curved line, consequently any reduction in the size of the wire rope does not mean a correspondingly reduced reading upon the ammeter.

Secondly, on p. 84, 7th edition, of S. P. Thomson's "Dynamo Electric Machinery," we find the following statement: "The permeability curves differ in detail even in different specimens of the same kind of iron"; thus, on account of differences in composition, a wire rope of exactly the same sectional area at different parts may give two different readings upon the ammeter.

Thirdly, according to Prof. Ewing, in his 3rd edition of "Magnetic Induction in Iron and other Metals," p. 212, we find the following: "Hardened steel, being stretched, differs in magnetism from its piece unstretched—*i.e.*, the presence of a small amount of pulling increases the susceptibility to magnetisation, the presence of a fairly large amount of pulling load reduces the susceptibility.

Thus, as the load on a hauling rope is continually changing when travelling up or

down a shaft, its susceptibility to magnetisation is also changing, and consequently the readings upon the ammeter are also changed proportionately. This effect is quite considerable, as is seen on examining the curves in Prof. Ewing's book.

Fourthly, the wire rope would have to be passed through the solenoid at a perfectly constant rate to obtain true readings, which is practically impossible to obtain under ordinary working conditions. This is necessarily due to the fact that the permanent magnetism in a steel rope gradually increases the longer the rope remains in the solenoid. As we are dealing with very small periods of time, represented by the time between each reversal of magnetism, any change in the rate at which the wire is being passed through the solenoid will effect the magnetic properties of the steel wire, and consequently the readings on the ammeter.

In conclusion, Mr. McCann's invention, to be of any practical value, has to register very small decreases in the area of a wire rope such as those caused by corrosion or broken internal wires which have not come apart, and, as there are so many factors tending to give incorrect readings upon the ammeter, it is impossible, as Mr. McCann states, that "the self-induction is increased and the current falls exactly in proportion to size of the rope so introduced."

Mr. E. Goffe: As a point of interest in connection with that apparatus of Mr. McCann's, I should like to mention that I recently saw a reference to a machine now in use at the Calcutta branch of the Royal Mint which from the description of its working appears to be a machine similar in principle to Mr. McCann's, and the use of that machine is to test the amount of the gold in coin passed through it. This machine appears to be superseding the delicate balances formerly used. I have not been able to get a full description of the machine, but from the short allusion in the Royal Mint's report it appears to be similar. It appears to me that if that machine is able to make such a delicate test as this, it is quite reasonable to expect that Mr. McCann's machine will accurately gauge the difference in thickness of a wire rope passed through it.

Mr. W. Martin Epton: I have seen Mr. McCann's instrument with 100 individual wires put into it. If one is drawn out there is unquestionably a marked difference in the reading of the ammeter, and apparently when you take a second one out there is as much more difference, and so on.

Mr. J. A. Vaughan: I think Mr. McCann is only too anxious to have a trial in a shaft, and we had arranged to do this when the instrument got broken. At the very earliest possible date which Mr. McCann can fix there will be a working test. I think there is a great deal in what Mr. Hughes says, more especially with regard to the speed of moving.

From the Journal of the Transvaal Institute of Mechanical Engineers.

Vol. V, No. 11. *1907.*

A FEW NOTES ON RESULTS OF TESTS OF WORN ROPES.

Paper by Mr. W. Martin Epton.

Mr. W. Martin Epton: I would like to preface my remarks by saying that this is not by any means a paper. These figures are the results of tests made in the Mines Department Mechanical Laboratory, and on going through them the other day I thought they might be of use to the resident engineers of the Rand. They are the results of 90 completed tests of worn ropes. The question has often been asked whether a worn rope is as strong as the aggregate of its wires. I simply had intended to put the figures in the *Journal*, and to let them go at that, but the Secretary asked me to put it in the form of a short paper, so I hope you will not expect anything very elaborate.

WIRE ROPE CAPPING.

As a great deal has been written about the capping of ropes, especially in Great Britain, it may be of interest to the members to know what has been done in this direction in the Transvaal. The following are the results of experiments made by the author in the Mines Department Mechanical Laboratory during the past six or eight months:—

It will be seen that in very few instances is the capping as strong as the rope.

It may be mentioned that, with three exceptions, Nos. 613, 719 and 720, the experiments were conducted on capping actually in use on mines in the Transvaal.

In test No. 613, socket No. 604 was used, the rope being held in it by exactly the same method as is employed in the laboratory for testing ropes—*i.e.*, the wires were cleaned, turned, and a seal of white metal then run round them.

In experiments Nos. 719 and 720 the splice and clamps were specially prepared for test by one of the mines in order to determine the efficiency of the two methods of attachment; a piece of the same rope was used in each case, and the results showed very little difference between the two methods. The clamps used were those known as the "Crosby" pattern, which allow of a wide bearing on the rope; clamps which only bear on points have a great tendency to do damage.

It was found from these experiments that the ordinary socket was liable to have a very low efficiency owing to the metal not having been properly run in, thus allowing all the stress to come on to the large end of the cone, where the wires are turned over, thus causing them to break. In two of the socket tests, which gave a high percentage of efficiency (Nos. 604 and 672), the good results were due to the fact that the rope was considerably worn outside, and not inside the socket.

It will be noticed from the experiments that the percentage of efficiency varies from 71·34 per cent. to 100 per cent., this difference, I believe, being entirely due to the skill with which the work is done, and for this reason it is not desirable to use a method of attachment which is dependent upon the skill of the workmen, and of which there are no means of ascertaining the efficiency without testing it in a machine.

As it is impossible to inspect the interior of a socket after the rope is once run in, either before the rope is used or during its working life, the clamp and splice are both preferable to the socket attachment for the reason that they are easily inspected; if, however, it is impossible to use either of the latter attachments, and the socket must be used on account of its short length, then the best method is to first tin the wires without turning them back, and then make the white metal cone, which cone I have never known to fail, although used in considerably over 2,000 tests which have been made in the Government Laboratory.

The following figures are taken from complete tests of worn winding ropes made in the Mines Department Mechanical Laboratory; the results of a few of the tests were made public in the paper read by Mr. Vaughan and myself on "Wire Ropes Used for Winding, their Strength, and Some Causes of its Reduction"; but of the remainder by far the greater number have been made since the paper was read.

It will be noticed that of the 90 tests, in 49 instances the whole rope breaks lower than the aggregate of its wires, while in 33 cases it breaks higher, and 8 cases give almost the same results in each test. There appears to be a very large range of difference when the whole rope breaks higher than the aggregate of its wires, as also when it breaks lower; on the higher side the difference ranges between 20 per cent. to 2·7 per cent., and on the lower side from 29 per cent. to 2·1 per cent. It is interesting to note that the 6/1 construction of rope has a greater tendency, when worn, to break higher than the aggregate of its wires than any other construction; the majority of ropes, however, break lower than the aggregate of the wires, which confirms what was said in the previous paper, viz., that although it may happen in some cases there is considerable lateral support, yet this cannot be relied upon, and should not enter into any calculations that may be made as to the strength of the rope from the amount of wear on the wires.

These figures are put forward in the hope that they may, in some measure, help the resident engineers on the mines to determine the strength of a worn rope.

Number of cases in which the breaking load of the rope as calculated from the aggregate of its wires, tested in their most

worn parts, is **greater** *than the actual Breaking load of the rope:—*

Construction.	B.L. of rope calc. from the Aggregate of its Wires tested at their most worn parts. (Tons of 2,000 lbs.)	B.L. of rope as tested. (Tons of 2,000 lb.)	Difference (Tons of 2,000 lb.)	Percentage of difference.
9/12/△	38	36	2	5·3
do.	40	39	1	2·5
do.	27	19	8	29·6
12/6/1	64	57	7	10·9
12/6/1	68	52	16	23·5
9/6/1	74	73	1	1·3
do.	73	69	4	5·5
do.	33	31	2	6·1
do.	60	50	10	16·7
do.	63	59	4	6·3
8/6/1	59	51	8	13·6
do.	39	31	8	20·5
do.	56	54	2	3·6
do.	56	50	6	10·7
do.	48	46	2	4·2
do.	53	50	3	5·7
do.	45	42	3	6·7
do.	55	52	3	5·5
do.	50	44	6	12·0
do.	52	49	3	5·8
do.	50	46	4	8·0
do.	62	59	3	4·8
do.	59	53	6	10·2
do.	87	82	5	5·7
do.	59	56	3	5·1
do.	33	32	1	3·0
do.	59	51	8	13·6
do.	39	31	8	20·5
do.	56	54	2	3·6
do.	56	50	6	10·7
8/5/1	69	62	7	10·1
7/5/1	58	55	3	5·2
do.	45	43	2	4·4
do.	42	36	6	14·3
do.	51	44	7	13·7
9/3/-	46	39	7	15·2
do.	44	36	8	18·2
do.	58	53	5	8·6
do.	39	37	2	5·1
do.	56	48	8	14·3
do.	61	54	7	11·5
8/4/-	34	32	2	5·9
6/1/-	31	30	1	3·2
do.	31	30	1	3·2
do.	47	46	1	2·1
do.	45	42	3	6·7
do.	43	39	4	9·3
do.	43	40	3	7·0
do.	34	33	1	2·9

In eight cases, not included above, the difference was less than one ton of 2,000 lb.

Number of cases in which the breaking load of the rope as calculated from the aggregate of its wires, tested in their most worn parts, is **less** *than the actual Breaking load of the rope:—*

Construction.	B.L. of rope calc. from the Aggregate of its Wires tested at their most worn parts. (Tons of 2,000 lbs.)	B.L. of rope as tested (Tons of 2,000 lb.)	Difference (Tons of 2,000 lbs.)	Percentage of difference.
9/8/△	35	36	1	2·8
8/6/1	44	48	4	8·3
do.	45	47	2	4·3
do.	40	42	2	4·8
7/5/1	37	40	3	7·5
do.	39	40	1	2·5

Construction.	B.L. of rope calc. from the Aggregate of its Wires tested at their most worn parts. (Tons of 2,000 lbs.)	B.L. of rope as tested (Tons of 2,000 lbs.)	Difference (Tons of 2,000 lbs.)	Percentage of difference.
do.	35	36	1	2·8
do.	40	46	6	13·0
6/1/	35	36	1	2·8
do.	30	33	3	9.1
do.	27	32	5	15·6
do.	23	25	2	8·0
do.	33	40	7	17·5
do.	35	39	4	10·3
do.	25	26	1	3·8
do.	30	37	7	18·9
do.	44	45	1	2·2
do.	41	43	2	4·7
do.	44	48	4	8·3
do.	28	32	4	12·5
do.	33	36	3	8·3
do.	22	24	2	8·3
do.	36	37	1	2·7
do.	22	25	3	12·0
do.	31	38	7	18·4
do.	28	29	1	3·4
do.	32	40	8	20·0
do.	21	25	4	16·0
do.	31	38	7	18·4
do.	48	51	3	5·9
do.	42	45	3	6·7
do.	40	43	3	7·0
do.	39	42	3	7·1

THE PRESIDENT: We are all very much indebted to Mr. Epton for the very interesting notes which he has laid before us this evening. I should like to ask Mr. Epton how many wires he tests to arrive at the aggregate when he is making comparisons of tests of single wires, and the aggregate of the whole rope. How does he account for such a large difference between tests 1 and 2 which he mentions in the first two examples of the sockets? It seems to me to be a larger difference than one can account for by ordinary workmanship, and I should like to know whether he has any explanation to offer.

Mr. W. MARTIN EPTON: The explanation is simple. If you look at that capping you will see that the metal has never been run into the interstices between the wires. To ascertain the aggregate of the wires, usually two strands are taken, and the wires are tested at the most worn places. The whole rope is cut in two, one part being tested as a whole rope, the other part being tested for the strength of the aggregate of its wires. I may say that this capping, which I did some considerable time ago, had an efficiency of only 50 per cent.; probably Mr. Johnson knows where it came from.

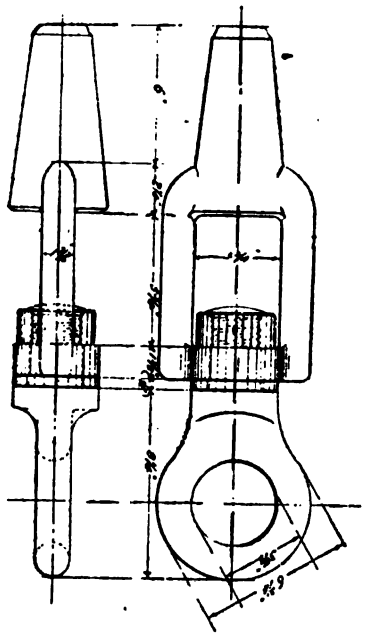
Mr. R. B. GREER: I have listened to this paper with a great deal of interest, because when I was at Home last I found it was a subject which was engaging the attention of both rope users and rope makers, and they found, as Mr. Epton did, that the cone-shaped attachment gave the best results. I should like to ask Mr. Epton if he could tell us the best composition to run in to produce that result. A number of experiments have been made at Home to obtain a material that would go into the small end of the cone, and yet be hard enough to avoid risk of its loosening by softening when the strain came on. They have found that the cone-shaped attachment produces far better results than either splicing or clamping.

Mr. W. MARTIN EPTON: I may state that I have used three or four different kinds of metal, and that I buy mine in the local market. I buy the cheapest, provided it has a low melting point. I have tested it three or four times for melting temperature, and find it the best.

THE PRESIDENT: I think Mr. Epton might give us the name. It would be valuable information.

Mr. W. MARTIN EPTON: It is ordinary magnolia metal. I have tried two or three kinds of magnolia, and they all hold perfectly well. But pure magnolia has the lowest melting point.

Mr. E. J. LASCHINGER: In connection with this paper, most of you may have seen in a recent number of *Engineering* an account of the failure of the stay wires of a Marconi antennæ tower. This tower was 400 ft. high, and built up in sections. In a gale one of the socket fastenings failed,



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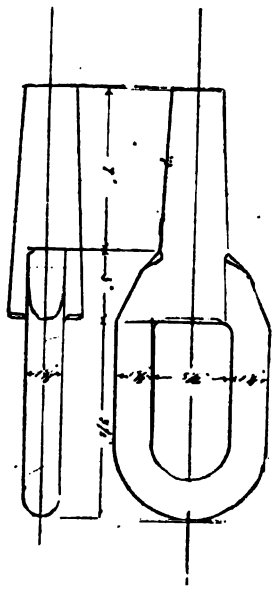

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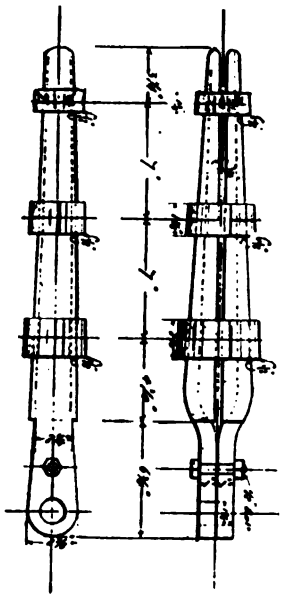

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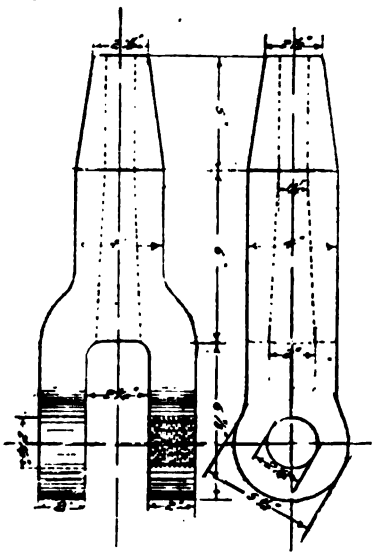

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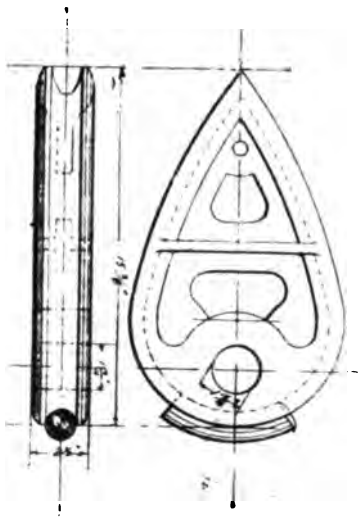

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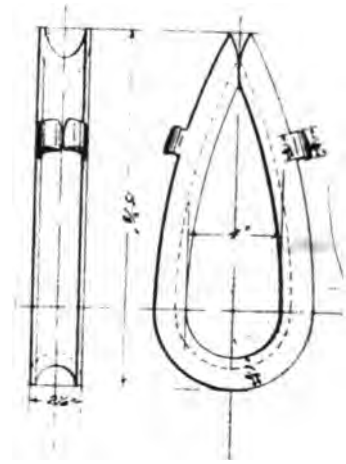
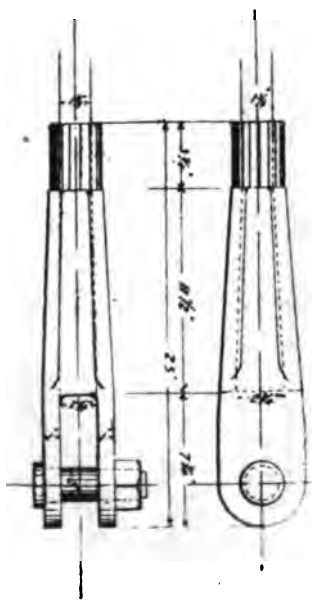
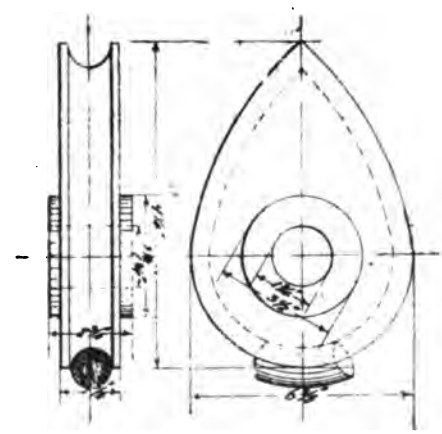


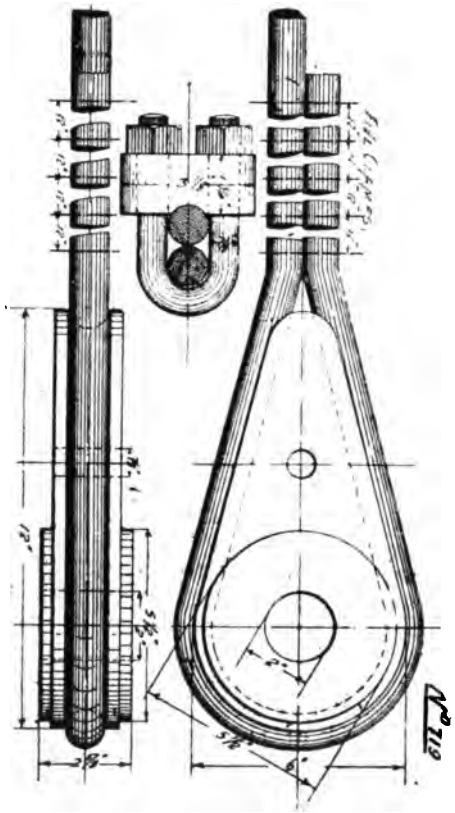
Nos. 672-673.
Length of Splice, 3 ft. 2 in.



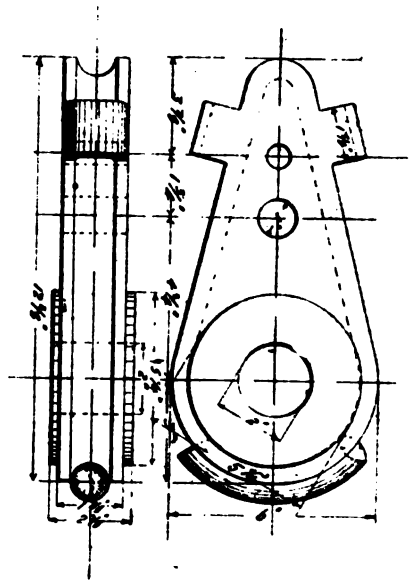
No. 680.



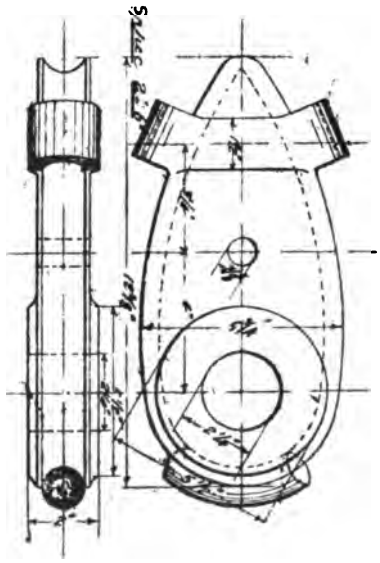
No. 681.
Length of Splice, 2 ft. 6¼ in.



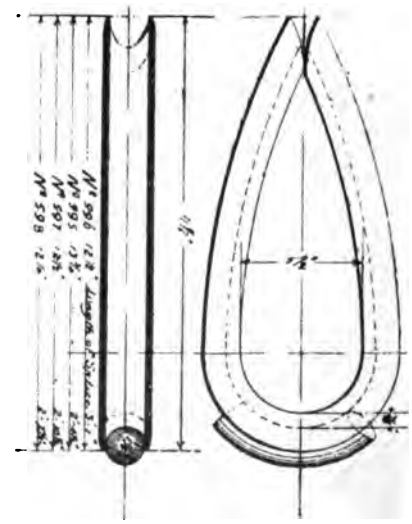
No. 719.



No. 720.
Length of Splice, 3 ft. 6in.



No. 725.



No. 746.
Length of Splice, 2 ft. 3½ in.

and as soon as that happened the whole tower came down. The socket was a tapered one, run in with metal, and investigation showed that the failure was due to the metal not having been run in properly. Similar sockets in other places held very well. Attention was drawn to the fact that these sockets were very good, and gave good results if they were properly made. The metal used for pouring into the sockets was zinc, and although that has not a low melting point, it seemed to do very well. One of the dangers of a high melting point is that it may alter the structure of the wires.

I am very sorry that the discussion on this paper is to be closed this evening, because it is impossible for one to both look through the drawings in a few minutes and listen to the reading of that paper and pretend to grasp anything more than just a few points. I do not see why we should be bound by hard and fast regulations with regard to short papers, and miss the chance of a valuable discussion, and, if in order, I move that this paper remain open for discussion at future meetings.

THE PRESIDENT (Mr. H. H. JOHNSON): Mr. Laschinger is quite in order to move that the discussion remain open. That has been our practice in the past.

Mr. W. MARTIN EPTON: When I first began to make these cones I was afraid of the temperature of the metal, and used a composition with bismuth in it. I sent Home for it, and paid 6s. or 8s. per pound. But as this price was excessive I tried other metals.

Mr. J. F. COOK: I should like to ask Mr. Epton if he could give us a little more information about these ropes. If we had the breaking stress of the rope when it was new, and the size, it might give us some idea of the deterioration and the reason for the great variation in the loss of strength. Could he also give the calculated strength of the wires? I suppose Mr. Epton has these figures, and if they were given as well the information might be more valuable.

Mr. W. MARTIN EPTON: I can do that at the next meeting, leaving the manufacturer's name out. I am afraid I could not give what the rope would break at originally. I could only give what the makers say it would break at.

Mr. W. THOMAS: Mr. Epton is to be thanked for placing these figures before us, but I am sorry that he has not introduced more matter of a controversial nature in connection with his remarks.

I agree with Mr. Cook that to make the figures more valuable they should be accompanied by further information.

A very frequent cause of the reduction in strength of ropes working on these fields is internal corrosion, and it occurs to me that in many of the cases cited, where the actual breaking stress of the worn rope is less than the aggregate strength of the wires, this result may be due to corrosion.

Another reason for so many ropes breaking at a lower stress than that of the aggregate of the wires may be found in the amount of lubrication contained in the centre core of the ropes. When ropes are laid up in such a way that the internal core can be kept lubricated by the external application of grease or oil, the gripping of the wires against the core is prevented. When, after a short period of work, the core of the rope becomes dry, the wires are held in position more readily, and assuming an equal amount of wear on the wires, the rope with the dry core should have a greater breaking stress than the rope with a well lubricated core. In cases where wires have broken, whether due to brazes or other causes, I have only noted two instances in which either end has been pushed back, so that the lay of the wire is affected in the strand on either side of the break. This appears to me to prove that there is a considerable gripping of the wires against the core in nearly every instance.

In Messrs. Vaughan and Epton's paper, "*Wire Ropes used for Winding, their Strength and Some Causes of its Reduction*," they show results of some tests of new compound construction ropes in which the amount deducted from the aggregate strength of the wires is as high as 17 per cent., whereas with a properly laid up rope in this construction, the amount to be deducted for spinning should not be greater than 10 per cent. Should the results tabulated include any ropes with such a high initial factor to be deducted for spinning, this fact should be mentioned, as it has an important bearing on the subject. It is not necessary for Mr. Epton to give the name of the makers in each case, but in cases where a rope has broken lower than the aggregate strength of the wires, I

should like Mr. Epton to state approximately the factor which he has found necessary to deduct from the aggregate strength of the wires in new ropes manufactured by the same firm.

In a new rope, a certain percentage must always be allowed for spinning, and, in cases where a rope is but slightly reduced in breaking strength from the original, the spinning factor may account for the actual breaking strength being less than the aggregate of the wires.

I am of opinion that, if a rope is not corroded, and there are no special governing conditions, after a certain amount of wear the gripping of the wires between the strands and against the internal core will always cause the breaking strength of the rope to be higher than the aggregate strength of the wires.

Mr. W. Martin Epton: It will be noticed that it is usually the 6 round 1 construction that breaks with a higher load than the aggregate of its wires; the point I wish to make is that although it sometimes happens that a rope will break higher than the aggregate of its wires, yet it is unsafe to calculate on this when assessing the strength of a worn rope.

Mr. W. Thomas: I should like to ask Mr. Epton, as a rope expert, whether he cannot usually tell, from the general appearance of the ropes, which will break higher, and which lower. I think, that in at least 75 per cent .of the cases, a higher or lower result can be predicted by an examination of the ropes.

Mr. W. C. Docharty: There is one point that occurs to me in connection with the capping of the rope. I wonder whether Mr. Epton has noticed when testing these ropes that, if the thimbles are made of some other metal than that of which the rope is constructed, it is possible that the galvanic action may have a detrimental effect on the rope at that end. It also occurs to me that, when the metal is run into these eyes, water getting into the socket may have a detrimental effect. I believe there is quite a prejudice against these sockets, and the question is whether the very sharp bending action of the rope at the top of the cone is not the reason why people object to them. Personally, I prefer the spliced rope round the eye, for the reason that one can always examine it properly, and I believe that, when the splice is properly put in, it is a more satisfactory method.

Mr. W. Martin Epton: With regard to the brass thimbles, I have only had three or four of them through my hands, and I have not noticed them particularly. As to water getting into a socket into which metal has been run, I do not see how the water can possibly get inside. The metal is part of the steel, and there is no room for the water to get in between it and the rope. With regard to the socket, Mr. Docharty is perfectly right. In working with a cage and chairs it will have a very detrimental effect. I am not advocating sockets, but there are cases where one cannot use anything but a socket, and in these circumstances I think the best socket to use is the one that is run in solid.

Mr. W. C. Docharty: With reference to the tinning of the wires, I am afraid that very few mines would go to that trouble. In a great many cases the rope is changed in a hurry, and that probably explains the condition of the two samples brought here. The metal had not gone through to the small part of the cone, thus causing the water to get in.

Mr. W. Martin Epton: That is not the socket I am advocating at all. It is the tinned socket that should be used. I would undertake to run one in in less than an hour if I had the material at hand. I may say that one of the mine managers within the last week ran a socket in that way, and I have been talking to-night to a gentleman who has seen it, and he said it appeared to be a very good job.

Mr. W. Thomas: With reference to the socket Mr. Epton has just referred to, I had an opportunity of inspecting it a few days ago. This socket was put on a rope working in a sinking shaft in order to shorten the connection between the rope and bucket as much as possible. It replaced the ordinary splice which had been in use previously. The method appears to me to make the best connection I have seen for this class of work, and the only possible danger will be due to the fact that, when the bucket is detached, the bending at the top of the connection may cause the wires to break at this point. Before a break occurs, however, the amount of wear can be readily detected in the daily inspection, and the rope can be re-socketed with very little trouble.

Mr. KENNETH AUSTIN: In connection with the tinning of the wires, to what cleaning process are they subjected?

Mr. W. MARTIN EPTON: The method in use is first to mix a bucketful of caustic soda and water. The boy then takes an ordinary brush, and after immersing the rope end in a bucket of clean hot water, brushes the grease off. It is then dipped into hydrochloric acid, which is washed off again, and then put into ordinary spirits of salts, after which it is dipped into molten soft solder. I may say that the whole of the work is done by natives, and the time taken does not exceed an hour.

TESTS OF CAPPINGS.

The following results accompany Mr. W. Martin Epton's paper, "A Few Notes on Results of Tests of Worn Ropes" printed in the June Number of the Journal:—

No. 548.—Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.115 in.
Circumference, 3.5 in.
Number of strands, 6.
Number of wires per strand, 7/5/1.
Diameter of wire, .116 in, .064 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of rope, 150,000 lbs.

TEST OF ROPE IN SOCKET.

Rope broke in socket at 107,010 lbs.
Efficiency of connection, 71.34 per cent.
See Diagram No. 548.
Condition of rope—Very slightly worn.

No. 549.—Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.115 in.
Circumference, 3.5 in.
Number of strands, 6.
Number of wires per strand 7/5/1.
Diameter of wire, .113 in., .057 in., .040 (unworn).
Class of main core, hemp.
Class of strand core, wire.
B.L. of rope, 111,900 lbs.

TEST OF ROPE IN SOCKET.

3 strands broke at 110,500 lbs. clear of socket.
Efficiency of connection, 100 per cent.
See diagram No. 549.

No. 604.—Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 0.947 in.
Circumference, 2.975 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .125 in. (unworn).
Class of main core, hemp.
Class of strand core, wire.
B.L. of rope, 73,965 lbs.

TEST OF ROPE IN SOCKET.

Rope pulled out of socket at 72,900 lbs. In this test, rope was taken through socket, severed about 1 foot from end, and strands opened out and wires cut to various lengths and bent over before metal was run in. This connection had been in use for some time. Efficiency of connection 98.56 per cent. See Diagram No. 604.
Condition of rope: Considerably worn.
Approximate size of worn wires, .078 in.

No. 680.—Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.106 in.
Circumference, 3.475 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .125 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of rope, 99,600 lbs.

TEST OF ROPE IN SOCKET.

Rope commenced to draw out immediately load was applied, and eventually pulled completely out at 73,000 lbs. Efficiency of connection 73.29 per cent. See Diagram No. 680.
Condition of rope: No wear.

No. 684. Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.09 in.
Circumference, 3.425 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .124 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of rope, 94,200 lbs.

TEST OF ROPE IN SOCKET.

Rope pulled out of socket at 67,000 lbs. Rope was drawn through socket and wires bent over end of same.
Efficiency of connection, 71.12 per cent.
See Diagram No. 604.
Condition of rope: Very slightly worn.
Approximate size of worn wires, .117 in.

No. 572.--Test of Rope in Socket:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 0.967 in.
Circumference, 3.0375 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire: .108 in. (unworn).
Class of main core, hemp.
Class of strand core, hard wire.
B.L. of rope, 72,598 lbs.

TEST OF ROPE IN SOCKET.

Rope pulled out of socket at 54,000 lbs.
Efficiency of connection, 74.38 per cent.
See Diagram No. 572.
Condition of rope: Very slightly worn.

No. 596.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.098 in.
Circumference, 3.45 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .125 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of steel per sq. in., 207,185 lbs.
B.L. of rope, 77,056 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

1 strand broke at last tuck of splice at 67,500 lbs.
Efficiency of connection, 87.60 per cent.
See Diagram No. 746.
Condition of rope: Considerably worn.
Approximate size of worn wires, .081 in.

No. 597.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.102 in.
Circumference, 3.4625 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .125 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of steel per sq. in., 214,158 lbs.
B.L. of rope, 92,870 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

2 strands broke at last tuck of splice at 89,600 lbs.
Efficiency of connection, 96.48 per cent.
See Diagram No. 746.
Condition of rope: Slightly worn.
Approximate size of worn wires, .118 in.

No. 628.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.107 in.
Circumfrance, 3.475 in.
Number of strands, 6.
Number of wires per strand, 8/6/1.
Diameter of wire, .103 in., .058 in., .061 in. (unworn).
Class of main core, hemp.
Class of strand core, wire.
B.L. of rope, 116,000 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

2 strands broke at last tuck of splice at 110,000 lbs.
Efficiency of connection, 94.83 per cent.
See Diagram No. 628.
Condition of rope: Very slightly worn.
Approximate size of worn wires (outside) .101 in.

No. 672.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.15 in.
Circumference, 3.625 in.
Number of strands, 6.
Number of wires per strand, 8/6/1.
Diameter of wire, .112 in., .062 in., .065 in. (unworn).
Class of main core, hemp.
Class of strand core, wire.
B.L. of rope, 106.000 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

3 strands broke clear of splice at 97,000 lbs
Note.—The difference in B.L. is probably due to wear.
Efficiency of connection, 100 per cent.
See Diagrams Nos. 672 and 673.
Condition of rope: Considerably worn.
Approximate size of worn wires (outside) .091 in.

No. 681.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.106 in.
Circumference, 3.475 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .125 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of rope, 99,600 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

The pin through thimble bent at 95,000 lbs. with rope and splice unbroken.
See Diagram of No. 681.
Condition of rope: No wear.

No. 719.—Test of Rope Clamped Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 0.979 in.
Circumference, 3.075 in.
Number of strands, 6.
Number of wires per strand, 7/5/1.
Diameter of wire, .099 in., .052 in., .041 in. (unworn).
Class of main core, hemp.
Class of strand core, wire.
B.L. of rope, 87,800 lbs.

TEST OF ROPE CLAMPED ROUND THIMBLE.

1 strand broke at 4th clamp from thimble at 78,800 lbs. The rope was fastened round thimble by five clamps, each 12 in. apart.
Efficiency of connection, 89.75 per cent.
See Diagram No. 719.
Condition of rope: No wear.

No. 720.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Same rope as No. 719.

TEST OF ROPE SPLICED ROUND THIMBLE.

1 strand broke at last tuck of splice at 78,000 lbs. (1 wire broke at first tuck of splice at 39,500 lbs.).
Efficiency of connection, 88.84 per cent.
See Diagram No. 720.

No. 725.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter,, 1.106 in.
Circumference, 3.475 in.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .124 in. (unworn).
Class of main core, hemp.
Class of strand core, soft wire.
B.L. of rope, 96,600 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

3 strands broke next to splice at 87,500 lbs. (Thimble was made of brass, and cracked during test).
Efficiency of connection, 90.37 per cent.
See Diagram No. 725.
Condition of rope: Very slightly worn.

No. 746.—Test of Rope Spliced Round Thimble:—

TEST OF WHOLE ROPE.

Construction, Lang's Lay.
Diameter, 1.058 in.
Circumference, 3.325 in.
Number of strands, 6.
Number of wires per strand, 8/6/1.
Diameter of wire, .101 in., .057 in., .060 in. (unworn).
Class of main core, hemp.
Class of strand core, hard wire.
B.L. of rope, 102,592 lbs.

TEST OF ROPE SPLICED ROUND THIMBLE.

1 strand broke at last tuck of splice at 101,000 lbs.
Efficiency of connection, 98.45 per cent.
See Diagram No. 746.
Condition of rope: Slightly worn.
Approximate size of worn wires, .086 in., .055 in.

EXPERIMENTS TO ASCERTAIN IF ANY DIFFERENCE EXISTS BETWEEN TESTS OF SHORT AND LONG SPECIMENS OF ROPE.

Construction, 8/6/1.
Length, 4 feet 4 inches (short specimen); 12 feet 10 inches (long specimen).
Diameter, 1.154 inches (short specimen); 1.154 inches (long specimen).
Breaking load (tons of 2,000 lbs.), 59.5 (short specimen); 59.6 (long specimen).
Remarks, 3 strands broke (short specimen); 5 strands broke (long specimen).
Construction, 8/5/1.
Length, 4 feet 4 inches (short specimen); 14 feet 10 inches (long specimen).
Diameter, 1.134 inches (short specimen); 1.134 inches (long specimen).
Breaking load (tons of 2,000 lbs), 50.2 (short specimen); 49.75 (long specimen).
Remarks, 3 strands broke and 3 wires in 4th (short specimen); 3 strands broke and several wires in 5th (long specimen).

TESTS OF ROPE WHEN NEW AND AFTER USE.

Tests of Rope when new and after 7 months' Use.

Particulars of rope:—
Date of manufacture, January, 1906.
Date of purchase, 6th March, 1906.
Coil number, 313.
Construction, Lang's Lay.
Length, 3,500 feet.
Diameter, 1 inch.
Number of strands, 6.
Number of wires per strand 7/5/1.
Diameter of wire, .100 in., .052 in., .041 in.
Class of main core, best hemp.
Class of strand core, hard wire.
Quality of steel, special improved plough.
Breaking load of steel per square inch, 257,800 lbs.
Breaking load of rope, 91,400 lbs.
Weight per foot, 1.576 lbs.
New rope broke at 91,907.2 lbs. Three strands broke.
Rope (after 7 months' use) broke at 87,292.8 lbs. Three strands broke.
Rope very slightly worn. No corrosion. Well lubricated.

Test of Rope when new and after 1 month's Use.

Particulars of rope:—
Date of manufacture, May, 1906.
Date of purchase, 7th January, 1907.
Coil number, 354.
Construction, Lang's Lay.
Length, 1,000 feet.
Diameter, 0.75 inch.
Circumference, 2.375 inches.
Number of strands, 6.
Number of wires per strand, 6.
Diameter of wire, .082 inch.
Class of main core, best hemp.
Class of strand core, best hemp.
Quality of steel, special improved plough.
Breaking load of steel per square inch, 256,000 lbs.
Breaking load of rope, 46,000 lbs.
Weight per foot, 0.7875 lb.
New rope broke at 45,400 lbs. Three strands broke.
Rope (after 1 month's use) broke at 44,643.2 lbs. Three strands broke. No wear or corrosion. Well lubricated.

Test of Rope when new and after 8 weeks' Use.

Particulars of rope:—
Date of manufacture, September, 1905.
Date of purchase, December, 1906.
Coil number, —
Construction, Lang's Lay.
Length, 3,030 feet.
Diameter, 1.25 inch.
Circumference, 4 inches.
Number of strands, 6.
Number of wires per strand, 12 (9 over 3).
Diameter of wire, .104 inch.
Class of main core, hemp.
Class of strand core, —
Quality of steel, best patent improved crucible.
Breaking load of steel per square inch, 201,600 lbs.
Breaking load of rope, 118,200 lbs.
Weight per foot, 2.67 lbs.
New rope broke at 121,400 lbs. Three strands broke.
Rope (after 8 weeks' use) broke at 118,400 lbs. Three strands broke. No wear or corrosion. Well lubricated, but a little rust outside.

Test of ropes when new and after 1 month's and 2 months' and 19 days' use.

Particulars of rope:—
Date of manufacture, September, 1906.
Construction, Lang's Lay.
Length, 3,700 feet.
Diameter, 1 inch.
Number of strands, 6.
Number of wires per strand, 6/1.
Diameter of wire, .111 in.
Class of main core, hemp.
Class of strand core, soft wire.
Quality of steel, —
Breaking load of steel per square inch, 268,800 lbs.
Breaking load of rope, 91,000 lbs.
Weight per foot, —
(Specimen taken from skip end of rope).
New rope broke at 82,000 lbs. Three strands broke. Rope (after 1 month's use) broke at 82,000 lbs. Three strands broke. No wear or corrosion. Well lubricated.

Another rope from the same mine, with exactly the same particulars as above, except that the core wire is hard instead of soft, gave the following results:—
New rope broke at 93,000 lbs. Two strands broke.
Rope (after 1 month's use) broke at 92,500 lbs. Two strands broke. No wear or corrosion. Well lubricated.
Rope (after 2 months' and 19 days' use) broke at 92,826 lbs. Two strands broke. No wear or corrosion. Well lubricated.

A FEW NOTES ON TESTS OF WORN ROPES.

PAPER BY MR. W. MARTIN EPTON, PRESIDENT.

Discussion.

The President submitted the following further particulars in connection with his paper:—Number of cases in which the breaking load of the rope, as calculated from the aggregate of the wires, tested in their most worn parts, is *less* than the actual breaking load of the rope:—

Construction.	B.L. Rope Calculated from Wires Tons.	B.L. Rope as Tested. Tons.	Difference in Tons.	Percentage of Difference	Size of Wires. New. Inch.	Size of Wires. Worn. Inch.	Amount of Corrosion.	Lubrication.	B.L. as per Maker's Certificate. Tons.	Usual Percentage Allowed by Maker for Spinning.
9/8 Δ	35	36	1	2·8	...	·082	Considerable	Deficient	54·00	None
8/6/1	44	48	4	8·3	·103·057	·075 ·047	do.	do.	58·90	8
do.	45	47	2	4·3	·103·057	·074 ·057	do.	Well	56·00	do.
7/5/1	37	40	3	7·5	...	·080	None	do.	56·00	None
do.	39	40	1	2·5	·113 ·058 ·040	·090	do.	do.	46·00	do.
do.	35	36	1	2·8	·113 ·058 ·040	·087	do.	do.	46·00	do.
do.	40	48	6	13·0	·113 ·057 ·040	·067 ·045	Considerable	Deficient	61·00	do.
6/1	35	36	1	2·8	·113	·103	None	Well	54·00	do.
do.	30	33	3	9·1	·108	·079	do.	do.	...	...
do.	27	32	5	15·6	·109	·078	do.	do.	32·48	8
do.	23	25	2	8·0	·109	·095	Slight	Deficient	31·50	do.
do.	33	40	7	17·5	·123	·084	None	Well	46·00	10—12
do.	35	39	4	10·3	·122	·086	Slight	do.	52·00	None
do.	25	26	1	3·8	·098	·096	None	do.	23·00	10
do.	30	37	7	18·9	·122	·073	do.	Deficient	52·00	None
do.	44	45	1	2·2	·126	·108	do.	Well	39·00	8
do.	41	43	2	4·7	·125	·098	Slight	Deficient	46·00	10—12
do.	44	48	4	8·3	·125	·099	do.	Well	50·50	T
do.	28	32	4	12·5	·110	·078	None	do.	51·00	...
do.	33	36	3	8·3	·110	·085	Slight	do.	40·00	T
do.	22	24	2	8·3	·125	·066	None	do.	46·00	10—12
do.	36	37	1	2·7	·125	·095	do.	do.	54·00	None
do.	22	25	3	12·0	·097	·071	do.	do.	27·68	10—12
do.	31	38	7	18·4	·125	·081	do.	do.	46·00	do.
do.	32	40	8	20·0	·125	·089	Excessive	Deficient	46·00	T
do.	21	25	4	16·0	·108	·066	Considerable	do.	46·00	do.
do.	31	38	7	18·4	·125	·081	do.	do.	46·00	10—12
do.	48	51	3	5·9	·125	·112	Slight	Well	52·00	T
do.	39	42	3	7·1	·125	·093	None	do.	46·00	10—12

TESTS OF WORN ROPES—*Continued.*

Number of cases in which the breaking load of the rope, as calculated from the aggregate of the wires, tested in their most worn parts, is *greater* than the actual breaking load of the rope:—

Construction.	B.L. Rope Calculated from Wires. Tons.	B.L. Rope as Tested. Tons.	Difference in Tons.	Percentage of Difference	Size of Wires. New. Inch.	Sizes of Wires. Worn. Inch.	Amount of Corrosion.	Lubrication.	B.L. as per Maker's Certificate. Tons.	Usual Percentage Allowed by Maker for Spinning.
9/12 Δ	38	36	2	5·3	...	·083	Considerable	Deficient	54·25	None
do.	40	39	1	2·5	...	·081	do.	do.	54·25	do.
do.	27	19	8	29·6	·099 ·0406	·060 ·025	do.	do.	54·60	do.
12/6/1	64	57	7	10·9	·073	New	None	Well	57·68	10
do.	68	52	16	23·5	·084	·071	do.	do.	68·32	10—12
9/6/1/	74	73	1	1·3	·106 ·072	·099 ·071	Slight	do.	73·80	8
do.	73	69	4	5·5	·107 ·072	·095 ·069	do.	Deficient	73·00	do.
do.	60	50	10	16·7	·101 ·066 ·072	·095 ·065	Excessive	do.	69·90	None
do.	63	59	4	6·3	·101 ·066 ·012	·089 ·066	...	...	72·80	do.
8/6/1	59	51	8	13·6	·103 ·057 ·057	·097 ·056	Slight	Well	60·00	8
do.	39	31	8	20·5	·103 ·057	·076 ·050	...	...	58·00	do.
do.	56	54	2	3·6	·103 ·057	·091 ·053	...	...	58·00	do.
do.	56	50	6	10·7	·101 ·057	·096 ·056	Slight Pitting	...	62·00	None
do.	48	46	2	4·2	·101 ·060	·072 ·053	Heavy	Deficient	46·00	do.
do.	53	50	3	5·7	·103 ·057 ·060	·089 ·052	Considerable	do.	58·00	8
do.	45	42	3	6·7	·101 ·057 ·061	·074 ·048	do.	do.	60·00	None
do.	55	52	3	5·5	·101 ·057 ·061	·074 ·048	do.	do.	60·00	do.
do.	50	44	6	12·0	·101 ·057 ·061	·087 ·050	Slight	do.	61·60	do.
do.	52	49	3	5·8	·0985 ·059	·081 ·060	None	Well	58·24	do.
do.	50	46	4	8·0	·102 ·056	·100 ·056	Slight	do.	57·50	8
do.	62	59	3	4·8	·102 ·060	·098 ·060	do.	do.	57·50	do.
do.	59	53	6	10·2	·102 ·060	·086 ·060	None	do.	57·50	None
do.	87	82	5	5·7	·089 ·051 ·052	New	do.	do.	...	...
do.	59	56	3	5·1	·103 ·057	·091 ·056	Slight	Deficient	56·00	8
do.	33	32	1	3·0	·101 ·057 ·061	·057 ·053	Considerable	do.	62·70	None
8/5/1	69	62	7	10·1	·114	·103	None	Well	74·60	do.
7/5/1	58	55	3	5·2	·112 ·056	·107 ·056	do.	do.	60·25	do.
do.	45	43	2	4·4	·113 ·057	·100 ·055	do.	do.	48·00	do.
do.	42	36	6	14·3	·100 ·052	·081 ·043	Excessive	Deficient	47·50	8
do.	51	44	7	13·7	·113 ·057 ·040	·093 ·056	...	...	60·34	None
9/3	46	39	7	15·2	·093 ·093	·071 ·088	Considerable	Deficient	56·00	10—12
do.	44	36	8	18·2	·095	·071	do.	do.	62·72	None
do.	58	53	5	8·6	·094	·080	Slight	do.	56·23	10—12
do.	39	37	2	5·1	·085 ·065	·063 ·065	Considerable	do.	41·00	do.
do.	56	48	8	14·3	·093	·078	None	Well	56·22	do.
do.	61	54	7	11·5	·093	·084	do.	do.	56·22	do.
8/4	34	32	2	5·9	·086 ·066	.080 ·066	Considerable	Deficient	34·00	do.
6/1	31	30	1	3·2	·100	·096	Slight	do.	38·00	None
do.	47	46	1	2·1	·125	·125	do.	Well	41·00	10—12
do.	45	42	3	6·7	·125	·115	None	do.	46·00	do.
do.	43	40	3	7·0	·109	·087	do.	do.	47·60	None
do.	34	33	1	2·9	·098	·097	do.	do.	38·00	do.

A FEW NOTES ON TESTS OF WORN ROPES.

PAPER BY MR. W. MARTIN EPTON.

Discussion.

Mr. W. S. THOMAS: Now that our President has completed his paper "A Few Notes on Results of Tests of Worn Ropes," by the addition of the tables, giving further information in connection with these tests, we are in a position to analyse the tests, and by so doing, show that the conclusions which Mr. Epton draws from the tests are diametrically opposite to the conclusions which should be drawn from them. Mr. Epton concludes that, "although it may happen in some cases that there is considerable lateral support between the wires in a worn rope, yet this cannot be relied upon, and should not enter into any calculation that may be made, as to the strength of the rope, from the amount of wear on the wires." A similar statement was made by Messrs. Vaughan and Epton in their valuable paper, "Wire Ropes Used for Winding: Their Strength, and Some Causes of Its Reduction," and I am very pleased that Mr. Epton has given us these results of tests, so that we may point out the fallacy of this assertion. Messrs. Vaughan and Epton state, in connection with this same subject, that "One can only go by averages" (Journal for April, 1905, page 214), and it will be seen from the tables herewith, that out of 71 tests which have been analysed, 80 per cent. show conclusively that there is a lateral support of the wires in worn ropes, and it is apparent that this factor should be considered and allowed for in calculating the breaking strain of worn ropes.

Earlier in this discussion, I expressed the opinion that "if a rope is not corroded, and there are no special governing conditions, *after a certain amount of wear*, the gripping of the wires between the strands, and against the internal core, will always cause the breaking strain of the rope to be higher than the aggregate of the wires," and after going into Mr. Epton's tests this opinion has been strengthened.

Mr. Epton has given us two tables, containing 71 tests, 29 of which show a higher breaking strain for the worn rope than the aggregate strength of the wires, tested in their most worn portions; and 42 show a lower breaking strain for the rope than the aggregate strength of the wires, tested in their most worn portions. From this latter table, two tests (Nos. 4 and 23) should be excluded, as they are tests of new ropes, and in the case of new ropes it is admitted that the breaking strain of the rope is less than the aggregate strength of the wires. These two examples were probably included in error, and have not been taken into account in the tables accompanying this contribution.

As Mr. Epton stated in the discussion, he is only able to give, in many cases, the breaking strains of the ropes as given in the makers' certificates, but together with the breaking strain given in the certificate, he has also given us the percentage which the makers of the rope usually deduct, and has noted the fact in cases where no deduction has been made. In calculating the original breaking strain of the ropes I have deducted 10 per cent. for spinning from the maker's certificate, in the cases stated as having had no deduction made originally, and the makers' breaking strains have been used in all cases where deductions have been made.

Ten per cent. is a fair average to deduct for spinning in any of the best known makers' ropes, in the constructions illustrated, and this percentage I have used arbitrarily to indicate the wear necessary to reduce the spinning factor to zero. It is quite impossible to determine the *exact* amount of wear which will reduce the spinning factor to zero, and no doubt this amount varies in different constructions, and is dependent upon varying conditions.

For the purpose of comparison, I have classified the results given by Mr. Epton in his table of cases, in which the aggregate of the wires is greater than the breaking load of the worn rope, into three classes, according to whether the amount of wear is more, or less, than 10 per cent. Assuming that in case the percentage of wear equals the spinning factor, a rope will break at a strain equal to the aggregate of the worn wires.

TABLE I.

In this table I have placed all of Mr. Epton's examples of ropes in which the breaking load of the worn rope is greater

than the aggregate strength of the wires tested in their most worn portions. It is assumed that the amount of wear apparent on the surface of the wires will be indicated by the percentage of difference between the original breaking load of the rope and the aggregate strength of the worn wires. This assumption may not be correct in all cases, but should be sufficiently so for forming a basis of comparison. Time does not permit me to work out the worn section of the wires and obtain the percentage of wear by comparing with the original sections, which would give an exact result. The percentage of difference between the original breaking load of the rope and the actual breaking load of the worn rope is given for comparision, with the assumed amount of wear. We find, in every case but two (Nos. 5 and 28) out of the 29 tests given, that the ropes are reduced by wear more than 10 per cent. Referring to the percentages I have given in the column, " Percentage of Difference between Original Breaking Load of Rope and the Breaking Load calculated from the Wires," it will be seen that the amount of wear necessary to reduce the spinning factor to zero is probably considerably over 10 per cent. This table shows that it is only in the case of a considerable amount of wear that we can expect the aggregate strength of the worn wires to be less than the actual breaking strain of the worn rope. It therefore follows that there may be a gripping of the wires in a worn rope even though the actual breaking load of the rope is less than the aggregate strength of the worn wires; and the following tables, 2, 3, and 4, have been compiled from these tests, showing that, in the majority of the examples, such is found to be the case. In three examples (Nos. 9, 14, and 16), the percentage of wear cannot be worked out owing to the fact that in the first case the original strain of the rope has not been given, and in the second and third cases the original breaking strains of the ropes which have been given are probably incorrectly stated, as in both cases the original breaking strains are lower than the breaking strains of the worn ropes. In considering the two cases before mentioned, of Nos. 5 and 28, in which the amount of wear is less than 10 per cent., in No. 5 the original breaking strain may have been given incorrectly, but this cannot be the case in No. 28, as the breaking strain here is given from a test of the new rope. This rope may possibly have been badly spun in the new portion which was tested, which would cause a lower breaking strain than that actually possessed by the rope.

TABLE II.

The tests placed in this table are taken from Mr. Epton's tables showing the number of cases in which the test of the worn rope is less than the aggregate of the wires. The amount of wear on the wires in the samples given in this table is assumed to be indicated by the percentage of difference between the original breaking load of the rope and the breaking load of the rope as tested, and in every case the amount of wear is less than 10 per cent., showing that the ropes in this set of tests were only slightly worn. The amount of wear has not been calculated in three instances. Nos. 14, 21 and 39, on account of the fact that there is a probable error in stating the breaking load of the new ropes. In every case except one (No. 26) the ropes break at less than $8\frac{3}{4}$ per cent. reduction from the aggregate of the wires, and if we assume that the original spinning factor was 10 per cent., this proves conclusively that there is a gripping of the wires in these cases. It is quite a logical assumption that had these ropes been more worn at the time of testing, the aggregate strength of the wires would have become less than the actual breaking load of the rope. In the one case, No. 26, the original factor necessary to be deducted for spinning may have been very high, as the construction is one not generally adopted. Should this assumption be correct this rope can also be classed as having the spinning factor reduced by wear.

TABLE III.

The tests placed in this class are from Mr. Epton's table showing the cases in which the test of the worn rope is less than the aggregate of the wires. The table shows the number of tests in which the wear, as indicated by the percentage of difference between the original breaking load of the rope and the breaking load of the rope as tested, is greater than 10 per cent. In every case, however, the percentage of difference between the aggregate strength of the worn wires and the breaking load as tested is less than 8 per cent., which shows a reduction in the *original spinning factor of* 10 *per cent.*, and indicates a gripping of the wires. The tests

shown in this table we might naturally expect to find in Table I, and the assumption is, that there is some other condition affecting these tests which is not shown. Five out of the seven ropes are noted as being considerably corroded, which probably accounts for the results of these tests being different from our expectations. Two tests are noted as only being slightly corroded, so the reason of these ropes not testing higher than the aggregate of the wires is not apparent.

TABLE IV.

The tests shown in this table are from Mr. Epton's table showing cases in which the breaking load of the worn rope is less than the aggregate of the worn wires. In this table the wear is greater than 10 per cent., assuming the wear to be indicated by the percentage of difference between the original breaking load of the rope and the breaking load of the rope as tested. The tests in this table do not indicate a gripping of the wires, as in every case the difference between the aggregate of the worn wires and the ropes as tested is greater than the original spinning factor. There are only 14 tests in this list, constituting but 20 per cent. of all the tests submitted. The results given in this table can only be accounted for as those having special governing conditions. Five out of the 14 tests, or 35 per cent., are noted as being either excessively or considerably corroded. The construction in some cases may cause a poor test in the worn rope, and it is interesting to note that 66 per cent. of the samples in the 6, 9 and 3 construction are in Table IV. No doubt Mr. Epton has information which would clear up the mystery in regard to some of the tests, and in his reply I trust that he will let us know whether this is the case. I think that I recognise one or two " old friends " in the list.

Considering the tests in all of the tables, we find that only 20 per cent. can be considered as dangerous during the time they are working under proper inspection.

In Table I, all the ropes show a higher breaking strain than indicated by the amount of wear which has taken place.

In Table II, the spinning factor is reduced, and the greatest error which could be made in assessing the breaking strain from the amount of wear is 3 per cent., if the amount of wear is as indicated by the difference between the original breaking strain and the tested breaking strain.

In Table III, an error of as great as 7 per cent. might be made in case the amount of wear on the wires is found to be the same as the percentage of difference between the original breaking load of the rope and the test breaking load.

In Table IV, all the ropes are dangerous according to the tests as given here, and I trust that Mr. Epton in his reply will let us know whether these results were indicated by the appearance of the ropes.

From an analysis of these tests it will be apparent that the *actual* breaking load of the rope should be stated in the maker's certificate, as a considerable error may be made in assessing the wear when only the aggregate strength of the wires is given.

Reviewing the tests as a whole, we find that of the sixty-nine tests analysed 80 per cent. indicate a gripping of the wires, and only 20 per cent. indicate that there has been no gripping. We must therefore conclude that the gripping of the wires in a worn rope is a factor to be considered. The results prove that under normal conditions, as a rope wears, the spinning factor is reduced, and it is safe to assume that after a certain amount of wear has taken place, the rope will break higher than the aggregate strength of its wires.

Mr. STOKES: The very useful results furnished by the author of the many tests carried out in connection with worn ropes and rope fastenings should commend themselves with much interest to all who are interested in the subject of wire ropes and are responsible for their safe keeping and upkeep. The form of fastening which is now being used for testing ropes at the Mines Department Laboratory has had a thorough test, not only in this laboratory, but for many years has been used by others as the most secure form of attachment throughout the laboratories in other parts of the world where rope testing is carried out. The idea so far as the writer is concerned emanates from the necessity of employing an efficient method of securing the cable ends of some bridges built on the suspension system in America, and mention is made of it some ten years ago in proceedings of the American Institution of Civil Engineers. The efficiency of this plan for securing the rope-end is fully demonstrated in the results

TABLE No. I.

Mr. Epton's number of cases in which the *breaking load of the rope* as calculated from the *aggregate of the wires*, tested in their most worn parts, is *less* than the actual breaking load of the rope. The amount of wear which has taken place in the wires of the rope has been calculated so as to show that in nearly every case this is greater than 10 per cent.

Number of Test in Table Counting from Top	Construction	B.L. of Rope calculated from Wires	B.L. of Rope as Tested	Percentage of Difference	Probable actual B.L. of Rope as calculated from Maker's Certificate	Percentage of difference between original B.L. of Rope and B.L. calculated from Wires	Percentage of difference between original B.L. of Rope and B.L. of Rope as Tested	Amount of Corrosion	Remarks.
1	9/8/Δ	35	36	2·8	48·6	27·9	25	Considerable	
2	8/6/1	44	48	8·3	58·9	25·2	18	do.	
3	do.	45	47	4·3	56·0	19·6	16	do.	
4	7/5/1	37	40	7·5	50·4	26·5	20	None	
5	do.	39	40	2·5	41·4	5·7	3	do.	
6	do.	35	36	2·8	46·4	15·4	13	do.	
7	do.	40	46	13·0	54·9	27·1	16	Considerable	
8	6/1	35	36	2·8	48·6	27·9	25	None	
9	do.	30	33	9·1	—	—	—	do.	
10	do.	27	32	15·6	32·48	16·8	1·4	do.	
11	do.	23	25	8·0	31·5	26·9	20	Slight	
12	do.	33	40	17·5	46·0	28·2	13	None	
13	do.	35	39	10·3	46·8	25·2	16	Slight	
14	do.	25	26	3·8	23·0	—	—	None	Probable error in B.L.
15	do.	30	37	18·9	46·8	35·8	20	do.	
16	do.	44	45	2·2	39·0	—	—	do.	Probable error in B.L.
17	do.	41	43	4·7	46·0	10·8	6	Slight	
18	do.	44	48	8·3	50·5	12·8	5	do.	
19	do.	28	32	12·5	51·0	45·0	35	None	
20	do.	33	36	8·3	40·0	17·5	10	Slight	
21	do.	22	24	8·3	46·0	52·1	47	None	
22	do.	36	37	2·7	48·6	25·9	23	do.	
23	do.	22	25	12·0	27·6	20·5	9	do.	
24	do.	31	38	18·4	46·0	32·6	17	do.	
25	do.	32	40	20·0	46·0	30·4	13	Excessive	
26	do.	21	25	16·0	46·0	54·3	45	Considerable	
27	do.	31	38	18·4	46·0	32·6	17	do.	
28	do.	48	51	5·9	52·0	7·6	1	Slight	
29	do.	39	42	7·1	4·6	15·2	8	None	

TABLE No. II.

Compiled from Mr. Epton's table showing number of cases in which the *breaking load of the rope* as calculated from the *aggregate of the wires*, tested in their most worn parts, is *greater* than the actual breaking load of the rope. This table shows the number of cases in which the ropes, when tested, were less than 10 per cent. reduced in breaking strain, and the deduction for spinning is either the same or less than the original deduction.

Number in Mr. Epton's List Counting from Top	Construction	B.L. of Rope calculated from Wires	B.L. of Rope as Tested	Percentage of Difference	Probable actual B.L. of Rope as calculated from Maker's Certificate	Percentage of difference between original B.L. of Rope and B.L. calculated from Wires	Percentage of difference between original B.L. of Rope and B.L. of Rope as Tested	Amount of Corrosion	Remarks.
6	9/6/1	74	73	1·3	73·8	–0·2	1·0	Slight	
7	do.	73	69	5·5	73·0	2·0	5·5	do.	
9	do.	63	59	6·3	65·6	3·9	10·0	—	
12	8/6/1	56	54	3·6	58·0	3·4	6·8	—	
14	do.	48	46	4·2	41·4	—	—	Heavy	Probable error in B.L.
17	do.	55	52	5·5	54·0	–1·8	3·7	—	
19	do.	52	49	5·8	52·4	0·7	6·4	None	
21	do.	62	59	4·8	57·5	—	—	Slight	Probable error in B.L.
24	do.	59	56	5·1	56·0	–5·0	·0	do.	
26	8/5/1	69	62	10·1	67·2	–2·6	7·7	None	
27	7/5/1	58	55	5·2	54·2	–6·5	–1·4	do.	
28	do.	45	43	4·4	43·2	–4·0	0·4	do.	
33	9/3	58	53	8·6	56·2	–3·1	5·6	Slight	
34	do.	39	37	5·1	41·0	4·8	9·7	Considerable	
37	8/4	34	32	5·9	34·0	·0	5·9	do.	
39	6/1	47	46	2·1	41·0	—	—	Slight	Probable error in B.L.
40	do.	45	42	6·7	46·0	2·1	8·7	None	
41	do.	43	40	7·0	42·9	·0	6·7	do.	
42	do.	34	33	2·9	34·2	0·5	3·5	do.	

TABLE No. III.

Compiled from Mr. Epton's table showing the number of cases in which the *breaking load of the rope* as calculated from the *aggregate of the wires*, tested in their most worn parts, is *greater* than the actual breaking load of the rope. This table shows the number of cases in which the ropes, when tested, were more than 10 per cent. reduced in breaking strain, but which show the deduction for spinning to be less than the original deduction.

Number of Test in Table counting from top	Construction.	B.L. of Rope calculated from Wires.	B.L. of Rope as Tested	Percentage of Difference.	Probable actual B.L. of Rope as calculated from maker's certificate.	Percentage of difference between original B.L. of Rope and B.L. calculated from Wires.	Percentage of difference between original B.L. of Rope and B.L. of Rope as Tested.	Amount of Corrosion.	Remarks.
1	9/12/Δ	38	36	5·3	48·8	22·1	26·2	Considerable	
2	do.	40	39	2·5	48·8	18·0	20·0	do.	
15	8/6/1	53	50	5·7	58·0	8·6	13·7	do.	
16	do.	45	42	6·7	54·0	16·6	22·2	do.	
20	do.	50	46	8·0	57·5	13·0	20·0	Slight	
25	do.	33	32	3·0	56·5	41·5	43·3	Considerable	
38	6/1	31	31	3·2	38·0	18·4	21·0	Slight	

TABLE No. IV.

Compiled from Mr. Epton's table showing the number of cases in which the *breaking load of the rope* as calculated from the *aggregate of the wires*, tested in their most worn parts, is *greater* than the actual breaking load of the rope. This table shows the number of cases in which the actual breaking load of the rope is reduced by more than the probable original amount necessary to be deducted for spinning.

Number of Test in Table counting from top.	Construction.	B.L. of Rope calculated from Wires.	B.L. of Rope as Tested	Percentage of Difference.	Probable actual B.L. of Rope as calculated from maker's certificate.	Percentage of difference between original B.L. of Rope and B.L. calculated from Wires.	Percentage of difference between B.L. of Rope and B.L. of Rope as Tested.	Amount of Corrosion.	Remarks.
3	9/12/Δ	27	19	29·6	49·2	45·1	61·3	Considerable	
5	12/6/1	68	52	23·5	68·3	0·4	23·8	None.	
8	9/6/1	60	50	16·7	63	4·7	20·6	Excessive	
10	8/6/1	59	51	13·6	60	1·6	15·0	Slight	
11	do.	39	31	20·5	58	32·7	46·5	—	
13	do.	56	50	10·7	55·8	-0·3	10·3	Slight	
18	do.	50	44	12·0	55·5	9·9	20·7	do.	
22	do.	59	53	10·2	51·8	—	—	None	Probable error in B.L.
29	7/5/1	42	36	14·3	47·5	11·5	24·2	Excessive	
30	do.	51	44	13·7	54·3	0·6	18·9	—	
31	9/3	46	39	15·2	56·0	17·8	30·3	Considerable	
32	do.	44	36	18·2	56·4	21·9	36·1	do.	
35	do.	56	48	14·3	56·2	0·3	14·5	None	
36	do.	61	54	11·5	56·2	-7·8	3·9	do.	

obtained, but there are a few particulars which appear to the writer as requiring some demonstration, and I submit the following questions to the author in the hope that he may be able in the near future to supply the information required. Can the author ascertain to what extent the individual wires are gripped solely by the natural adhesion of the metal to the wires? To what extent does the efficiency of this form of fastening depend upon the form given to it? The intense compressive effect produced by a heavy load on the tapered form used must necessarily increase the pressure upon the wires near the surface of the metal and adds additional security. The writer believes that this tapered form is largely responsible for the very satisfactory results obtained owing to the soft nature of the mineral surrounding the wires. It would seem desirable in order to prevent any misunderstanding in the matter to ascertain the limits within which safe working conditions can be obtained from this tapered form. The writer, as well as the author, has seen this fastening pull through the dies when testing rope strands due to insufficient taper. On the other hand, if the writer's contention is correct, there must be a limit beyond which the increased gripping effect is not sufficiently produced to be of any material benefit owing to too much taper. I might further suggest that the author should test similar fastenings cast in a parallel form to show the length of wire necessary to be embedded before rupture occurs. Can the author say whether acids usually met with in the shafts along the reef have any deteriorating influence on the adhesive properties of the metal covering, and to what extent? I have mentioned these few matters because if it is intended that this form of fastening should be generally adopted the information requested will be valuable to those designing sockets for its use, and its service is of such vital importance that a thorough understanding should be arrived at before attempting any deviations from standard forms in cases where it might be advantageous to do so.

The writer has observed, in the examination of worn ropes, that in many cases of ropes termed "slightly worn" the wires just as often break at their unworn sections as the worn, and instances are known where considerably worn ropes have had nearly the whole of the wires broken at their full sections. Whether this is due to the lateral adhesion of the wires to themselves or some inherent mechanical alteration of the particles of the steel in excessively worn ropes is a matter which has, as yet, not been sufficiently explained. The kinetic effects through shocks manifested under working conditions call for undoubted reliability of the elastic properties of the component parts of ropes, and the writer is of opinion that these properties should be given a prominent place in the results of tests on worn ropes and a comparison made of the individual wires with those of unworn ropes as well as the results obtained in this respect from whole rope tests. It is usually understood that after certain use the breaking load of wires is increased, but I doubt whether this increase is sufficient to make up for the loss and fatigue due to brittleness. It may be said that the hemp core interior provides for elongation of a rope by allowing a reduction in its diameter, and this is no doubt true to a certain extent, but the fact that a rope does become dead is well recognised and is regarded as a warning of distress by many careful engineers, who take precautions in some instances to have the rope immediately removed. It is at this stage in the life of a rope, when the results of the elastic tests are of importance, and also the permanent set of individual wires after breaking should receive consideration and be compared with the unworn new wires. The author is at present placed at some slight disadvantage in making such tests at the present time on account of being obliged to record elongations by hand, but it is hoped that in the near future he will be supplied with more suitable appliances for placing these important matters on a more satisfactory basis.

A FEW NOTES ON RESULTS OF TESTS OF WORN ROPES.

Paper by W. Martin Epton, M.I.Mech.E. (President).

Discussion.

Mr. Ostreicher said it had been mentioned in connection with this paper that a worn wire had a greater strength than it ought to have. If they reduced locally the section of a test bar, the flow of the material was restricted, and the result was a higher breaking stress. That was quite a well-known fact, and was only what could be expected.

Mr. H. H. JOHNSON asked whether he understood Mr. Ostreicher to say that if a piece of steel 1 sq. in. in section were taken and one-sixteenth of an inch planed off it, that the tensile strength per square inch would be increased.

Mr. OSTREICHER replied that that was proved to be the case. If two bars were taken—one 3ft. long, and the other 2in. long—it would be found that the 2in. bar would give a greater stress per square inch than the 3ft. bar. The stress depended entirely on the ratio of length to diameter. They could only compare two tests when the specimens were geometrically equal.

PROFESSOR DOBSON thought that Mr. Ostreicher, in his original statement, was referring to two pieces of the same length, say, one with 1 sq. in. section, and the other with 1¼ sq. in. section turned down at the centre of 1in., in which case results would be obtained as he described.

Mr. W. S. THOMAS said that, according to Mr. Ostreicher's statement, a wire of long length would give the same result as a wire of short length. They would hardly notice any variation.

Mr. OSTREICHER said that they would find there was a variation. In 2in. as against 1ft. there was a difference of about 10 per cent. If a hole were punched in a bar, and the bar tested, it was found that the perforated bar had a greater strength per square inch than the solid bar.

Contributed since the Meeting:

In looking through the material in my possession, I find that the first part of my statement, viz.:—

> "That a local reduction of the cross section of a test bar, such as a round groove turned out or a hole drilled or punched, increases the tensile strength, and reduces the elongation and contraction, which is explained by the restriction of the flow of the material, and, therefore, geometrically similar test bars can be employed,"

has been established by repeated experiments.

In connection with the second, I must acknowledge that I was wrong. I find that the contraction decreased with the length, which means that shorter bars are apparently weaker, which is also proved by more recent experiments. But even here the law is very complicated, depending much on the shape of cross sections, which is more marked as we approach the yielding point, or the limit of rapid flow.

I beg to enclose a few tables which prove my statement that only geometrically similar test bars can be compared.

In regard to the increase of tensile stress of wires compared with the material not drawn or annealed wire, it must be borne in mind that the molecules of the wire are rearranged after the drawing thus: The outer skin has a tendency to expand, in consequence of which the inner layers are more restricted from contracting than before, which produces greater strength per square inch.

KIRCALDY'S EXPERIMENTS WITH ROUND IRON BARS OF 1 INCH DIAMETER.

Material treated.		Breaking strength Lbs. per sq. in.	Contraction Per cent.
Low Moor Iron of the hardest quality.	(1) Plain Cylinder, 1 in. diam.	64,600	51·00
	(2) Cylinder 1 in. diam., groove reduced to about .73 in. diam.	90,800	8·03
	(3) Plain Cylinder, about .73 in. diam. ...	68,400	49·23
Govan forged wrought iron.	(1) Plain Cylinder, 1 in. diam.	71,400	40·71
	(2) Cylinder, 1 in. diam. with groove reduced to about ·73 in. diam.	98,100	13·77
	(3) Plain Cylinder, about ·73 in. diam. ...	71,400	36·02
Govan rolled wrought iron.	(1) Plain Cylinder, 1 in. diam.	57,400	47·38
	(2) Cylinder, 1 in. diam. with groove reduced to about ·73 in diam.	70,300	21·27
	(3) Plain Cylinder, about ·73 in. diam. ...	62,000	46·91

CHIEF ENGINEER BARBA OF CRUESOT.

No.	Thickness. in.	Width. in.	Ratio.	Strength Tensile, lbs. sq. in.	Elongation. per cent.	Remarks
1	·399	·787	1·98	60,600	29·5	Mild steel.
2	·393	2·350	6·02	58,600	35·0	
3	·400	3·860	9·81	57,100	40·0	
1	·514	·787	1·53	34,100	51·5	Copper.
2	·513	2·350	4·57	33,800	55·2	
3	·516	2·990	7·61	22,800	50·0	

The above are extracts from Bach's book on Strength and Elasticity, 1st edition, converted into English measure.

WATERTOWN ARSENAL TESTS ON SINGLE-RIVETED DOUBLE-BUTT JOINTS.

OPEN HEARTH STEEL PLATES, $\frac{5}{8}$ IN. THICK, $\frac{1}{4}$ IN. COVER PLATES.

IRON RIVETS, MACHINE DRIVEN, HOLES DRILLED.

Material of Plates. Strength lengthwise =56 500 lbs. per sq. in. crosswise =56 500 lbs. per sq. in. Elastic Limit =33 000 lbs. per sq. in. Elongation in 10 in. =26% Reduction of area =55% (Each result the mean of two tests).

Size of Rivet and of Hole inch.	Pitch of Rivets inches.	Tension on Nett Section of Plate.	Tension of net section Strength lengthwise.
$\frac{7}{16}$ and $\frac{9}{16}$	$1\frac{5}{8}$	64,900	1·15
	$1\frac{3}{4}$	61,320	1·08
$\frac{11}{16}$ and $\frac{3}{4}$	$1\frac{3}{4}$	64,800	1·14
	$1\frac{7}{8}$	62,900	1·11
	2	67,050	1·18
	$2\frac{1}{8}$	63,300	1·12
$\frac{13}{16}$ and $\frac{7}{8}$	$1\frac{7}{8}$	64,900	1·14
	2	67,400	1·19
	$2\frac{1}{8}$	65,700	1·16
	$2\frac{1}{4}$	63,600	1·12
	$2\frac{3}{8}$	62,400	1·10
	$2\frac{1}{2}$	62,500	1·10
$\frac{15}{16}$ and 1	2	68,000	1·20
	$2\frac{1}{8}$	66,400	1·17
	$2\frac{1}{4}$	64,200	1·13
	$2\frac{3}{8}$	63,700	1·12
	$2\frac{1}{2}$	62,300	1·10
	$2\frac{5}{8}$	63,400	1·12
	$2\frac{3}{4}$	63,100	1·11
	$2\frac{7}{8}$	61,100	1·08

THE PRESIDENT (Mr. W. Martin Epton), in reply to Mr. Johnson as to the results obtained in testing different length of ropes, stated that the results of certain tests he had made were to be found in the Journal. In testing pieces of rope 20ft. long and 14ft. long the results were practically the same. Mr. Ostreicher's statement would not be borne out in connection with the wire rope, because there, several wires, and not a compact body, were being tested.

Mr. J. A. VAUGHAN thought that Mr. Ostreicher had explained his meaning. It was a well-known fact that if materials were stretched above the elastic limit, and not broken, and allowed time to recuperate, it would then show a higher elastic limit than it ever had before. That was the only fact he knew which bore out anything like the statement Mr. Ostreicher had made that evening.

A FEW NOTES ON TESTS OF WORN ROPES.

PAPER BY MR. W. MARTIN EPTON, President.

Discussion.

Mr. A. S. OSTREICHER asked whether the President had yet had time to carry out his promised experiments to see how the length of a rope influenced the breaking load. A rope was a compound body and it would be interesting to have the desired particulars.

The President said he had already made tests of this description and would have pleasure in supplying the required information in his reply to the discussion.

A FEW NOTES ON RESULTS OF TESTS OF WORN ROPES.

PAPER BY MR. W. MARTIN EPTON, M.I.Mech.E., President.

Author's Reply to Discussion.

The writer would like to thank all those who have taken part in the discussion on this paper, more especially Mr. Brodigan and Mr. Thomas; the former for kindly contributing the results of his practical experience with the method of capping as described in the original paper, and the latter for the trouble he has taken in analysing the results of experiments made with worn ropes.

Most of the other questions were replied to when asked, with the exception of Mr. Ostreicher's inquiry as to whether there was any apparent difference in the results of tests of a short or long specimen of rope; this point he will find is answered in the original paper, where the results of some experiments are given, but this part of the paper was, unfortunately, only published in the August number of the Institute's Journal. It may, however, be stated that, up to the present, the writer has found no difference in results of tests due to the length of the specimen.

Regarding Mr. Thomas's criticism, he has taken both Mr. Vaughan and the writer very severely to task for making the assertion that lateral support of the wires in a worn rope should not be taken into account when calculating its strength—the writer

would like to point out most emphatically that it was never asserted that lateral support did not sometimes exist, for it was known that it did exist, but its amount is of such a variable nature that the writer is still of the opinion that it is much safer to ignore it when calculating the strength of worn ropes. After all Mr. Thomas's calculations, involving possibilities and probabilities, he admits that he cannot account for 20 per cent. of results obtained by his method of calculation, which means on the Witwatersrand mines some 70 winding ropes could not be assessed if his methods were used, and the difficulty would be to find out to which 70 of the 350 in use his rule should not apply.

The writer can agree generally with Mr. Thomas's statement in regard to lateral support (which amounts to an axiom, and applies to new ropes as well as worn ones, although not, perhaps, to the same extent), that if there are no special governing conditions, *i.e.*, if there is perfect efficiency of the rope regarded as structure, then the rope will give a higher breaking load than the aggregate of its component wires. The limit of advantage will be reached when the rope fractures at a plane section paralled to its axis, and the advantage will then be in proportion to the areas of steel broken through. This can be calculated in the case of a new rope, but would be very difficult in worn ones, remembering that the lay of the wires is at an angle with the said plane.

From full consideration it appears that Lang's Lay ropes are slightly less efficient when worn than Ordinary Lay, but only from a theoretical point of view; the point to be established is that, given perfect conditions, the rope, whether new or old, ought to break higher than its wires. Included in the perfect conditions are the following:

(1) Every wire must be of the same grade, strength, and ductility, and laid in such a manner that it takes its proper share of the load and no more; this holds for both new and worn ropes, and postulates an impossibility.

(2) The deterioration in every respect of each wire must be the same, which looks like another impossibility.

Turning now to Mr. Thomas's basis of calculation, he says: "From an analysis of these tests it will be apparent that the *actual* breaking load of the rope should be stated in the maker's certificate, as a considerable error may be made in assessing the wear when only the aggregate strength of the wires is given." The writer agrees with the whole of this, but after Mr. Thomas has made this statement, he takes the trouble to try to prove that every rope has lateral support, and that he can assess the amount of such support, proceeding to use figures in which he admits a considerable error may be made, and this, to the writer, seems an extraordinary waste of time for a busy man who has not time to work out the areas of the wires and so give exact results.

Now it is this very uncertainty of being able to determine the exact amount of allowance, even if the areas are calculated, that should be made for spinning and lateral support, that induced Mr. Vaughan and the writer to make the statement that lateral support should not be taken into account when assessing the strength of a worn rope.

To illustrate the point, in Table No. 1, the writer has been able to calculate the areas in 19 cases, it being impossible to do so in the remainder owing to various reasons, such as grooving, corrosion, etc.; of these 19, four show no lateral support, as the percentage of wear is less than the percentage of reduction in breaking load; 8 of the remainder show errors in Mr. Thomas's figures varying from 5 per cent. to 26 per cent., while the others are within 3 per cent.

Table No. 2 deals with only 19 ropes, in three of which Mr. Thomas is quite right in saying that there is probably error, but the error is in the maker's certificate. Of the remaining ropes, one contains wires both brittle and grooved, and another in which the wires were phenomenally brittle, so that no nice calculations can apply. Mr. Thomas states that no greater error than 5 per cent. could be made if the amount of wear was as indicated by the difference between the original breaking stress and the tested breaking stress. Well, of the remaining 12 ropes, in Table 2, of which the writer was able to calculuate the areas, 7 do not agree with the relationship by considerably more than his 3 per cent., whilst the remaining 5 come within 2 per cent. of his figures, which would increase the 3 per cent. error to a 5 per cent. error.

In Table No. 3, seven ropes appear in which Mr. Thomas states that the spinning

factor of 10 per cent. has not been eliminated, and to show the fallacy of making calculations in the way he has done, it is found that, in 3 of the 7, on calculating the original areas and the breaking load per square inch of steel, the breaking load given in the maker's certificate is some tons higher than the grade of steel in the rope ever permitted; in 2 of the other cases it has not been possible to calculate the areas on account of corrosion, whilst the sixth rope is a mysterious one, known in the Government Records as the "Meyer and Charlton." It will, therefore, be seen that not one of these ropes could have been so nicely assessed as to allow for from 5 per cent. to 10 per cent. lateral support.

In Table No. 4, Mr. Thomas has classed ropes which he considers would be dangerous if his method of assessment were used, and there were no governing conditions. The writer has looked into all these cases, and, with the exception of two ropes (one of which had split wires, and the other was the famous "Meyer and Charlton") he has been able to find no special governing conditions.

In conclusion, the writer would like to point out that Mr. Thomas's initial mistake in his calculations was to assume that every rope had a spinning factor of 10 per cent., as it varies in the writer's own experience from 2 per cent. up to as high as 16 per cent., whilst a case is quoted in the *Practical Engineer's Pocket Book*, in which a spinning factor of 25 per cent. occurred in a rope having a breaking strain of 330 tons. Even Mr. Thomas's calculations for his own ropes are not always correct, although I must say that, from a purchaser's point of view he is usually on the right side. This is stated merely to prove the great difficulty in assessing the breaking load of a rope from the aggregate strength of its individual wires. A further difficulty that presents itself is at what time in the life of a rope does the spinning effect wear off; *i.e.*, suppose no deterioration occurs in the wires, when will the breaking load of the rope be equal to the breaking load of the aggregate of its wires? The writer certainly had, at one time the idea that after a little work the breaking load became nearer the aggregate of the wires, and in order to prove this point he made five tests for the Rope and Safety Catch Commission (the results of which were published in the August, 1907, number of the Institute's Journal), but in none of these experiments did the breaking load of the rope, after being under strain for what would have been considered a sufficiently long enough period for this to occur, become greater than it was when new. The number and diversity of tests made in this connection is not, however, put forward as giving a conclusive result.

If we assume, for the sake of argument, that Mr. Thomas is able to calculate exactly when the spinning had been reduced by say 3 per cent. to 5 per cent., or even when it is quite eliminated, the advantage to be gained would be extremely small. Take it that he calculates that wear and tear has reduced the strength of the rope 20 per cent. from 50 to 40 tons, and then allows that the spinning factor has been reduced from 10 per cent. to 3 per cent., this would mean the addition of about $3\frac{1}{2}$ tons to the original estimated breaking load of the rope, which is now to be taken to the credit of the worn rope.

It appears to the writer that many other factors are likely to affect the strength of the rope, which can be more exactly determined, than can the $3\frac{1}{2}$ tons convenient addition arrived at by guess work, and he would much prefer that whatever lateral support, if any, exists, should remain as a reserve of strength in the rope, and not taken to the credit side in attempting to settle its breaking load.

Extract of Paper from the Journal of the Transvaal Institute of Mechanical Engineers, Vol. VII., No. 7, 1909.

RECORD KEEPING AS APPLIED TO THE RESIDENT ENGINEER'S DEPT. OF A MINE.

PAPER BY H. L. TEMPLER, M.I.MECH.E. (MEMBER).

Winding Ropes.—The writer considers that the usual Rope Record Books to be obtained from various publishers leave much to be desired as regards accessibility of information, and strongly advocates the use of a separate and distinct book for each winding rope considered separately as a unit designated by the manufacturer's own number.

An ordinary foolscap book bound in cloth and costing about one shilling is all that is required, and contains the following:—

1. Original manufacturer's certificate.
2. Daily inspection report signed by the person who is delegated for this duty.

This report to be written on right-hand page only.

3. Details of any occurrences to shaft or skip, to be entered opposite its date on left-hand page.
4. Engineer's Monthly Inspection Report to be entered towards the end of the book.

Figure 3 illustrates a sample page of this book. While the rope and consequently the book is in use, the same is labelled by the shaft and compartment; when, however, the rope is discarded, a new book having taken the place of the one in question, the designation of the shaft and compartment is removed, and the manufacturer's number substituted.

Figure 4 shows a useful chart which shows at a glance the following information:—

1. Shafts and compartments.
2. Manufacturer's name of any rope either in use or that has been discarded.
3. Manufacturer's number of ditto.
4. Life of ditto.
5. If transferred from one compartment to another, which and where.
6. Dates of periodical tests by Government Laboratory.
7. Results of above tests.

Foot tons hoisted by each rope can, if desired, be entered on this chart against the rope at date of removal.

Assuming all the above data to have been accurately kept, it would be possible for any person to quickly obtain a general idea of the state of any particular rope in use, and also to gauge by the law of chance the probable life of a rope on a particular compartment, but on a question so important to the engineer as the safety of a particular rope, it becomes apparent that more minute data would be of material assistance.

Assuming a rope to have been working say eight months, and that 6 or 7 wires had fractured at different times at different places and that the rope had been shortened on two occasion and in addition turned end for end, upon the rigger after a daily inspection reporting another broken wire at a particular spot, it is difficult to at once locate in one's mind's eye where this has taken place with reference to the previous broken wires reported.

Accordingly the writer submits Figure 5, which shows at a glance the more minute history of any particular rope.

Many engineers take measurements monthly of the diameter of their ropes with a view to gauging the decrease of area due to wear and tear, and these measurements, if accurately taken and recorded, undoubtedly form a guide to determine the decreasing factor of safety.

Owing to the fact that any section of a rope is unlikely to be a true circle, it appears to the writer to be a waste of time to use any form of calliper.

By taking 10 circumferences a very great degree of accuracy can be obtained.

To illustrate this, let us assume that a rope measures 1in. in diameter or $10\ \Pi = 31{\cdot}41$ in.
It would be difficult to read a diametrical measurement closer than $\frac{1}{64}$ in.

Accordingly the next size below 1 in. would be $\frac{63}{64}$ in. $= {\cdot}984$ in. or $10\ \Pi = 30{\cdot}9$ in.
A difference of *half an inch*.

The writer finds that $10\ \Pi$ measurements can be taken almost as quickly as with a gauge and that it is quite easy to obtain measurements to a $\frac{1}{16}$ in.

This means that diameters varying $\frac{1}{500}$ in. can be accurately measured by this means.

At each monthly inspection, or oftener if considered necessary, the $10\ \Pi$ measurements are taken at as many places in the rope as may be desired, and these measurements are tabulated in the form shown in Figure 5.

At each succeeding inspection the measurements as obtained are noted at the exact position measuring from the skip. Accordingly, if between two inspections the rope has been shortened, a new line is drawn underneath the one previously in use

Fig. 3. ROPE RECORD BOOK.

	1908 Dec.	No. 2 Shaft West Compartment.	
	1	In Good Order	J.B.
	2	,,	J.B.
	3	1 Wire broken 1115′ and 1 at 1117′	J.B.
	4	Just the same	J.B.
	5	,,	J.B.
	6	,,	J.B.
Skip off Track going down 1000′ 11.15 p.m. Delay 5 hrs. 55 mins.	7	,,	J.B.
	8	,,	J.B.
	9	1 Wire broken 900′ and 1 at 1125′	J.B.
	10	Shaft undergoing repairs	J.B.
	11	,,	J.B.
	12	,,	J.B.
	13	,,	J.B
	14	,,	J.B.
	15	1 Wire broken 1000′	J.B.
	16	Just the same	J.B.
Skip off track 600 going down. Delay 3 hrs. 10 mins.	17	,,	J.B.
	18	,,	J.B.
	19	,, Monthly Examination	J.B.
	20	,, Greased	J.B.
	21	,,	J.B.
	22	,,	J.B.
	23	,,	J.B.
	24	,,	J.B.
	25	,,	J.B.
	26	,,	J.B.
Skip off Track 100′ coming up. Delay 7 hrs. 30 mins.	27	,,	J.B.
	28	,,	J.B.
	29	,,	J.B.
	30	,,	J.B.
	31	,,	J.B.

)RD.

MPTON.

LABORATO

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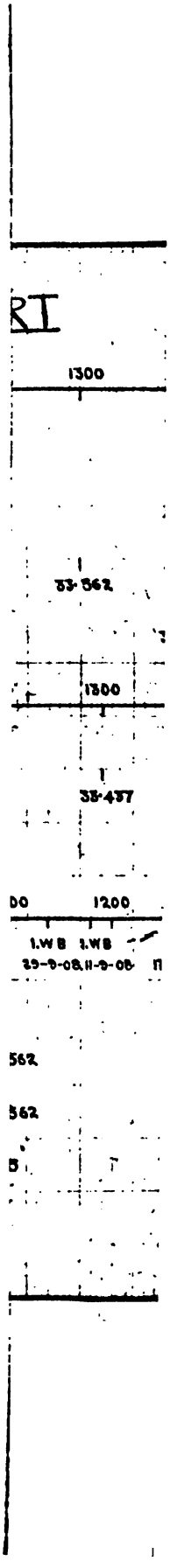
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1.W.B
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11-9-08
562
562

LIFE OF WIRE ... DRUMS).

No.	Quality of Steel and Construction of Rope.	Breaking Stress of Steel per sq. inch in 2,000 lb. tons.	Breaking Stress of Rope in 2,000 lb. tons.	Circumference of Rope.	Weight of Rope per 100 feet.	Length put into use.	Why taken off.	Stretch whilst in use.	Cost per Load per 100 feet of Rope in use.	Cost per Load per Length of Rope in use.
					lbs.	feet		ft. in.	Pence.	Pence.
1	Crucible	95/100	84·67	5in.	425·41	1,1	Condemned.	30 0	·013	·147
2	,,	95/100	84·67	,,	425·41	1,1	,,	30 0	·013	·147
3	Plough	112/123	102·06	,,	406	1,1	,,	11 9	·014	·150
4	,,	112/123	102·06	,,	406	1,1	,,	11 9	·014	·150
5	,,	112/123	102·06	,,	406	1,0	,,	16 9	·016	·163
6	,,	112/123	102·06	,,	406	1,0	,,	16 9	·016	·163
7	,,	112/123	102·81	,,	404·75	1,0	,,	12 9	·009	·092
8	,,	112/123	102·81	,,	404·75	1,0	,,	12 9	·009	·092
9	,,	112/123	102·06	,,	429	1,0	,,	12 9	·010	·097
10	,,	112/123	102·06	,,	429	1,0	,,	12 9	·010	·097
11	,,	112/123	102·81	,,	407·96	1,0	,,	12 6	·005	·048
12	,,	112/123	102·81	,,	407·96	1,0	,,	12 6	·005	·048
13	,,	112/123	102·81	,,	407·96	1,0	,,	12 0	·005	·050
14	,,	112/123	107·85	,,	422·54	1,1	,,	12 0	·005	·058
15	,,	112/123	107·85	,,	413·17	1,1	,,	10 0	·007	·077
16	,,	112/123	107·85	,,	422·31	1,1	,,	10 0	·007	·075
17	,,	112/123	102·06	,,	407	1,1	,,	3 0	·015	·161
18	,,	112/123	102·06	,,	407	1,1	,,	3 0	·015	·161
19	,,	112/123	102·81	,,	419·39	1,1	Hoisting stopped in this shaft 31/7/09.	10 6	·007	·079
20	,,	112/123	102·81	,,	419·39	1,1	do.	10 6	·007	·079
21	,,	131	120·6	,,	417·1	1,6	Condemned.	7 0	·011	·172
22	,,	131	120·6	,,	417·1	1,6	,,	7 0	·011	·172
23	,,	112/123	102·06	,,	411·03	1,4	,,	4 6	·019	·273
24	,,	112/123	102·06	,,	411·03	1,4	,,	4 6	·019	·273
25	,,	112/123	102·81	,,	417·85	1,6	,,	13 0	·007	·121
26	,,	112/123	102·81	,,	417·85	1,6	,,	13 0	·007	·121
27	,,	112/123	107·85	,,	429·19	1,6	,,	9 6	·011	·180
28	,,	112/123	102·81	,,	420	1,6	Taken off, and replaced 2/10/10.	9 6		
29	,,	112/123	102·81	,,	416·5	1,6	Condemned.	15 0	·006	·095
30	,,	112/123	102·06	,,	409·81	1,6	,,	9 0	·008	·120
31	,,	112/123	102·81	,,	420	1,6	,,	15 0		
32	,,	112/123	102·81	,,	414· 9	1,6	(At present in use)			
33	,,	112/123	102·81	,,	414·19	1,6	do.			

NOTE.—Ropes marked A are overla... taken off and put on same
NOTE.—An average maximum acce... of 1,104 hours.
calculating the actual factor of ... use ... ·122d.
Total loads hoisted ... 3...

LIFE OF WIR DRUMS).

No.	Quality of Steel and Construction of Rope.	Breaking Stress of Steel per sq. inch in 2,000 lb. tons.	Breaking Stress of Rope in 2,000 lb. tons	Circumference of Rope.	Weight of Rope per 100 feet.	tal ads ted, and ef.	Why taken off.	Stretch whilst in use.	Cost per load per 100 feet of Rope in use.	Cost per load per length of Rope in use.
				Inch.	Lbs.			Ft. ins.	Pence.	Pence.
1	Crucible	95/100	84·67	5	442·40	162	Condemned	20 0	·0078	·117
2	,,	95/100	84·67	5	442·40	162	,,	20 0	·0078	·117
3	Plough	112/123	102·81	5	401	911	,,	15 6	·006	·078
4	,,	112/123	102·81	5	401	911	,,	15 6	·006	·078
5	,,	112/123	102·06	5	428·61	214	,,	16 0	·006	·083
6	,,	112/123	102·06	5	428·61	214	,,	16 0	·006	·083
7	,,	112/123	107·85	5	421·30	383	,,	19 1½	·005	·067
8	,,	112/123	107·85	5	421·30	383	,,	19 1½	·005	·067
9	,,	112/123	107·85	5	424·30	270	,,	15 0	·012	·151
10	,,	112/123	107·85	5	424·30	379	,,	20 9	·006	·078
11	,,	112/123	107·85	5	423·59	319	,,	14 0	·003	·046
12	,,	112/123	102·06	5	403·08	122	,,	14 9	·008	·104
13	,,	112/123	102·06	5	403·08	075	,,	13 6	·007	·095
14	,,	112/123	102·06	5	412·58	540	,,	8 0	·011	·152
15	,,	112/123	102·06	5	412·58	540	,,	7 0	·011	·152
16	,,	112/123	107·85	5	422·51	163	,,	12 9	·006	·074
17	,,	112/123	107·85	5	422·51		At present in use.			
18	,,	112/123	107·85	5	425·32		,,			

NOTE.—An average ma: January, 1910, hoisting
calculating the act nderlay on Drum.

LIFE OF WIRE ROPES

No.	Quality of Steel and Construction of Rope.	Breaking Stress of Steel per sq. inch in 2,000 lb. tons.	Breaking Stress of Rope in 2,000 lb. tons.	Circumference of Rope.	Weight of Rope per 100 ft.	Length put into use.	Depth hoisted from	ad et pe p.	Cost per load per length of Rope in use.
				Inches.	Lbs.	Feet.	Feet.		Pence
1	Plough	112/123	74·59	4⅛	283·2	1,686	1,200		·141
2	,,	112/123	74·59	4⅛	283·2	,,	,,		·132
3	,,	112/123	77·61	4¼	298	,,	,,		·066
4	,,	112/123	77·61	4¼	298	,,	,,		·060
5	,,	112/123	77·61	4¼	298	,,	,,		·053
6	,,	112/123	77·61	4¼	298	,,	,,		·058
7	Mild Siemens Martin	—	—	4½	—	,,	,,		—
8	,,	—	—	4½	—	,,	,,		—
9	Plough	112/123	77·61	4¼	306	,,	,,		·066
10	,,	112/123	77·61	4¼	306	,,	,,		·066
11	,,	112/123	77·61	4¼	306	,,	,,		·073
12	,,	112/123	77·61	4¼	306	,,	,,		·073
13	,,	112/123	69·45	4¼	311	1,886	1,400		·116
14	,,	112/123	69·45	4¼	311	,,	,,		·116
15	,,	112/123	77·61	4¼	307	,,	,,		·193
16	,,	112/123	77·61	4¼	307	,,	,,		·193
17	,,	112/123	77·61	4¼	307	,,	,,		·112
18	,,	112/123	77·61	4¼	307	,,	,,		·112
19	,,	112/123	69·45	4¼	310	,,	,,	6	·111
20	,,	112/123	69·45	4¼	310	,,	,,	6	·111
21	Crucible	95/100	78·62	5	407	2,220	,,	03	·075
22	,,	95/100	78·62	5	407	,,	,,	03	·076
23	,,	95/100	63·50	4½	312	1,900	,,	18	·346
24	,,	95/100	63·50	4½	312	,,	,,	17	·326
25	,,	95/100	78·62	5	407	2,000	1,7	07	·132
26	,,	95/100	78·62	5	407	,,	,,	06	·112
27	,,	95/100	78·62	5	402	2,185	,,	07	·170
28	,,	95/100	78·62	5	407	2,170	,,	05	·099
29	,,	95/100	78·62	5	407	2,220	,,	08	·170
30	,,	95/100	75·60	5	425·41	2,200	,,	06	·133
31	,,	95/100	75·60	5	426·45	,,	,,	07	·148
32	Plough	112/123	102·81	5	400	,,	,,	08	·173
33	Crucible	95/100	78·62	5	407	,,	,,	06	·106
34	Plough	112/123	102·06	5	406·62	2,220	,,	015	·337
35	,,	112/123	102·81	5	404·46	,,	,,	010	·215
36	,,	112/123	102·81	5	400·09	2,220	,,	006	·126
37	,,	112/123	107·85	5	417·84	2,500	,,	024	·598
38	,,	112/123	102·81	5	405·92	2,600	,,	015	·401
39	,,	112/123	102·81	5	405·92	2,600	,,	020	·521
40	,,	112/123	107·85	5	424·22	2,900	,,	—	—
41	,,	112/123	107·85	5	418·54	,,	,,	015	·429
42	,,	112/123	107·85	5	—	—	,,	—	—
43	,,	112/123	102·81	5	412·38	2,900	,,	012	·351
44	,,	112/123	102·81	5	412·38	,,	,,	012	·351
45	,,	112/123	102·81	5	411·03	,,	,,	015	·438
46	,,	112·123	102·81	5	406·27	,,	,,	015	·432
47	,,	112/123	102·81	5	406·55	,,	,,	—	—
48	,,	112/123	102·81	5	421·51	,,	,,	—	—

NOTE.—Where left blank records incomplete.

NOTE.—An average maximum acceleration of 10 feet per second per second ned for calculating the actual factor of safety.

NOTE.—Rope marked thus † put on Skip A and worked 266 hours, taken on Skip B and then worked 1,608 hours, or a total of 1,874 hours ads hoisted, 245,254. Stretch whilst in use 18 feet. Cost per lo feet in use, ·012d. Cost per load per length of Rope in use ·35(

LIFE OF WIRE ROPE (WHITING HOIST).

No.	Quality of Steel and Construction of Rope.	Breaking Stress of Steel per sq. inch in 2,000 lb. tons.	Breaking Stress of Rope in 2,000lb. tons.	Circumference of Rope.	Weight of Rope per foot.	Why taken off.	Stretch whilst in use.	Cost per load per 100 feet of Rope in use.	Cost per Load per Length of Rope in use.
				Inch			Feet	Pence.	Pence.
1	Flattened strand spec. flex. Plough.			4¾	4	Condemned.		·004	·096
2	Crucible	95/100	69·55	,,	36	A bad piece in rope 600 feet from end.		·007	·156
3	,,	95/100	69·55	,,	36	Condemned.		·004	·090
4	,,	95/100	63·50	,,	30	,,	38	·003	·076
5	Plough	112/123	69·45	4¼	2	Rope stranded about 400 feet from bottom.	25	·017	·394
6	,,	112/123	77·61	,,	3	Too short for 1,840 ft. level. In good condition	20	·011	·252
7	Flattened strand crucible			5	4	Condemned.	23	·007	·184
8	,,			,,	4	Damaged through accident.	24	·008	·204
9	,,			,,	4	,,	27	·027	·708
10	Crucible	95/100	70·15	,,	4	Condemned.	40	·009	·226
11	,,	95/100	70·15	,,	4	,,	47	·008	·201
12	,,	95/100	70·15	,,	4	,,	43	·008	·205
13	,,	95/100	70·15	,,	4	,,	41	·004	·100
14	,,	95/100	70·15	,,	4	Too short for 2,160 ft. level. In good condition	43	·004	·106
15	,,	95/100	70·15	,,	4	Condemned.	48	·0116	·336
16	,,	95/100	78·62	,,	4	Taken off to be used as tail rope.	45	·008	·240
17	,,	95/100	84·67	,,	4	Condemned.	55	·008	·229
18	,,	84	73·18	,,	4	,,	39	·015	·431
19	,,	95/100	84·67	,,	4	,,	49	·0049	·143
20	,,	95/100	78·62	,,	4	Accident to shaft.	22	·025	·716
21	,,	95/100	70·15	,,	4	Condemned.	30	·004	·121
22	,,	95/100	84·67	,,	4	,,	49	·010	·289
23	,,	95/100	84·67	,,	4	,,	51	·006	·160
24	Plough	112/123	102·81	,,	3	Too short for 2,520 ft. level. In fair condition.	31	·014	·399
25	,,	112/123	102·81	,,	4	Condemned.	37	·0097	·311
26	,,	112/123	102·06	,,	4	,,	34	·008	·273
27	,,	126	109·0	,,	4	,,	36	·008	·258
28	,,	112/123	102·81	,,	4	,,	40	·006	·205
29	,,	127	115·44	,,	4	,,	35	·009	·288
30	Extra Cable quality	105	90·96	,,	4	,,	55	·005	·176
31	Plough	112/123	102·81	,,	4	At present in use.			

NOTE.—Where left blank r second is

LIFE OF WIRE

No.	Quality of Steel and Construction of Rope.	Breaking stress of Steel per sq. inch in 2,000-lb. tons.	Breaking stress of Rope in 2,000-lb. tons.	Circumference of Rope.	Weight of Rope per 100 feet.	Length put into Use.	Depth Hoisted from	Diameter of	Cost per load per length of rope in use.
				inches.	Lbs.	Feet	Feet	Fe	Pence.
1	Crucible	95/100	84·67	5	426·45	1,100	500	1	·034
2	,,	95/100	84·67	5	426·45	1,100	,,	,	·034
3	Plough	112/123	102·06	5	407·80	1,250	,,	,	·147
4	,,	112/123	102·06	5	407·80	1,250	,,	,	—
5	,,	112/123	102·81	5	400·85	1,000	,,	,	·027
6	,,	112/123	102·81	5	400·85	1,000	,,	,	·086
7	,,	112/123	102·06	5	407·80	1,100	,,	,	—
8	,,	112/123	107·85	5	422·31	1,100	,,	,	·150
9	,,	112/123	107·85	5	422·31	1,100	,,	,	·062
10	,,	112/123	99·9	5	415	1,300	,,	,	·764
11	,,	112/123	107·85	5	424·30	1,300	,,	,	·142
12	,,	112/123	102·06	5	404·09	1,100	,,	,	·123
13	,,	112/123	102·06	5	404·13	1,100	,,	,	·123
14	,,	112/123	107·85	5	414·14	1,300	,,	,	·059
15	,,	112/123	107·85	5	414·14	1,300	,,	,	·059
16	,,	112/123	102·06	5	405·07	1,450	,,	,	·188
17	,,	112/123	102·06	5	405·07	1,450	,,	,	·188
18	,,	112/123	107·85	5	422·92	1,300	,,	,	·051
19	,,	112/123	107·85	5	422·92	1,300	,,	,	·051
20	,,	112/123	99·9	5	415	1,300	,,	,	·128
21	,,	112/123	99·9	5	415	1,300	,,	,	·128
22	,,	112/123	102·06	5	430·34	2,100	,,	,	·247
23	,,	112/123	102·06	5	404·56	2,100	,,	,	·186
24	,,	112/123	102·81	5	412·70	2,000	980 (from 16/5/10	,	·109
25	,,	112/123	102·06	5	401·70	2,000	980	,	·164
26	,,	127	115·44	5	416·66	1,680	,,	,	
27	,,	127	115·44	5	416·66	1,680	,,	,	

NOTE.—Rope marked thus ‡ put on Skip A and worked rum. taken off and put on same skip after a period of 90 d worked 4,200 hours or a total of 5,688 hours. Total 1,049.450. Stretch whilst in use 30 feet. Cost per feet in use ·0028d. Cost per load per length of rope in

and the skip is moved along a corresponding amount to that by which the rope has been shortened. Similarly, if the rope has been changed end for end between two inspections a new line is drawn and the skip goes to the other end of the line.

By this means it therefore follows that all measurements taken can be accurately entered on the spot in the rope to which they refer, no matter how many times the rope has been shortened or changed end for end.

Broken wires can also be entered up on this chart, so that the relative positions of each fractured wire can be seen at a glance with reference to one another.

A curve can also be plotted, for any particular construction of rope with the abscissae representing 10 Π d measurements and the ordinates shewing the theoretical breaking strain of the rope due to reduction of area.

The data for this curve will be found on page xxvii of the Report of the Rope and Safety Catch Commission.

From the Journal of the South African Institute of Engineers.... (Extract.)

Vol. IX., No. 7. *1911.*

THE KIMBERLEY SYSTEM OF HANDLING LARGE QUANTITIES OF GROUND IN THE MINIMUM OF TIME, WITH NOTES REGARDING THE LIFE OF WIRE ROPES.

Paper by Mr. Alpheus F. Williams.

WIRE ROPES.

An important problem in connection with the hoisting of large quantities of ground is the question of the life of hoisting ropes. Unfortunately it is one of the subjects upon which there is not much reliable statistical information, and even in cases where records have been accurately kept for a number of years, as has been done by the De Beers Company, the results obtained from ropes of the same construction, supplied by the same maker, and working under exactly the same conditions, vary so considerably that no definite conclusion can be arrived at as to the life of any particular rope.

(*Vide* 5 Tables regarding the Life of Wire Ropes.)

Generally speaking, the factors which affect the life of a rope at the Kimberley mines are:—

(1) The initial strength of the rope.
(2) The personal element, which includes possible initial damage to a rope by persons handling the rope before it is put into use, and also possible excessive stresses caused by carelessness on the part of the drivers.
(3) Chemical deterioration.
(4) Shocks.
(5) Stability of the rope in the shaft during winding operations.
(6) Surface wear of the ropes on the drums.
(7) Internal wear between the different elements of the rope.
(8) The design and lay out of the hoisting machinery and headgear.

INITIAL STRENGTH OF THE ROPE.

Taking these headings in order, the first, namely, the initial strength of the rope, is one of considerable importance. In all devices for carrying weights the engineer usually falls back upon his old friend, "the factor of safety" to make things secure. In the case of ropes the initial factor of safety varies considerably in different localities, and which is the most economical factor to adopt is perhaps a matter for experiment for each particular mine. An increase in the initial factor of safety over and above that which is considered absolutely necessary does not necessarily mean a more economical rope. Increase in factor of safety up to certain depths may be obtained either by the adoption of higher tensile steel, the diameter of the rope remaining the same, or by the selection of larger diameter ropes without adopting steel of higher tensile strength. In each of these cases, as might be expected, the initial cost of the rope is increased, but whether the life is increased in proportion to the price is a problem towards a solution of which the author hopes to make practical experiments in the near future.

Ropes have been ordered for each of the large mines of the De Beers Company. In the first case the ropes are being supplied of the same diameter, but having steel of a

different tensile strength from that of the ropes heretofore in use.

Following upon the results to be obtained from these ropes, figures will be obtained from ropes of larger diameters, and it is anticipated that some conclusion will be arrived at as to which will be the most economical factor of safety for the ropes to be used at the mines in question. By economical factor of safety is meant that factor which, if increased, will add more to the cost of the rope than is justified by the additional work which the rope will do.

PERSONAL ELEMENT.

All ropes are subject to personal treatment after reaching the mine. First they are handled by the staff whose duty it is to fit them in place, and after being put into use they are under the personal control of the man on the operating platform of the winding engine.

It is impossible to calculate how much the personal treatment may affect the life of a rope, but the failure of many ropes before their expected time can only be explained by concluding that they have had some unusual treatment.

The splicing of the rope may be done in such a way as to throw unequal loads upon the different strands, and a kink in the rope caused during the process of fitting may not apparently do much harm at the moment, but may sooner or later reveal itself by a local defect.

Excessive stresses due to bumps, shocks or unusual jerks due to drivers doing something unusual with their engines, all affect the life of a rope.

Breakdowns of a rope due to the personal element causes are naturally difficult to trace to their source as the delinquent is averse to reporting anything which he ought not to have done. In some cases the failure of a comparatively speaking new rope has been explained by the candid confession of carelessness or accident by one of the workmen, but in others the confession which might explain matters is never made.

CHEMICAL DETERIORATION.

Chemical deterioration, which is a most important factor at some mines, is practically unknown in the Kimberley mines.

SHOCKS.

Under this heading may be classed shocks imposed upon the rope by the conditions of working as distinct from those which may be caused by careless handling of the engine. Shocks of any sort, no matter from what cause, are detrimental to the life of a rope. A shock may be given to a rope by a high initial rate of acceleration of the load, by variations in the rate of speed during the hoist, by want of uniformity in the turning moment of the engine, by the skip running into and out of the tipping device at the surface, by the possibility of starting the trip with a slack rope, and by other causes. Some of these causes may be classed as belonging to the personal type. High rate of acceleration, the rate of tipping and righting the skip and slack rope at the beginning of the wind are entirely under the control of the engine driver.

All drivers in the employ of De Beers Company have instructions to take up all slack on their rope during the period required to fill the skip.

STABILITY OF THE ROPE IN THE SHAFT.

To hoist a load by means of a rope without making it take up some vibration during the trip is practically impossible. During each trip when the load is initially suspended it follows as a matter of course that there is a stretch in the rope, the amount of stretch depending upon the weight lifted and upon the elasticity of the rope. The weight when suspended is naturally in a state of unequal equilibrium, as far as up and down motion is concerned, any force acting on the load in a vertical direction sets up vertical displacements of a harmonic nature, and causes the well-known pumping action on the skip. During the hoisting trip variation in the turning motion of the engine, variations in the rate of acceleration, etc., all tend to produce this action. When the trip is commenced, and consequently when there is a long length of rope hanging in the shaft, this pumping action will probably have little detrimental effect, but during the trip the pumping motion which would do little damage to a long rope, has to be taken up by a length which is rapidly diminishing, and it is reasonable to assume that the stresses in the rope from this cause increase as the load nears the surface, owing to the volume of the

rope, and consequently its resilience becoming small.

Another evidence of unstability of a rope is the appearance of transverse or bowstring vibrations which always appear in a rope under hoisting conditions. These vibrations which are usually slow when the hoist commences rapidly increase in number and decrease in amplitude as the surface is approached.

Vibrations, no matter whether of the pumping or the transverse type, will affect the life of a rope.

An interesting point in connection with vibration in a rope, confirmed by Kimberley experience, is the fact that there is a critical velocity for each length of rope at which these vibrations become absolutely dangerous. At Kimberley mine, if the hoisting engine is run at a certain speed below the normal it is possible to make the rope flap from side to side of the hoisting compartment. Below and above this speed the disturbances are not excessive.

All vibrations in a rope, whether transverse or in the direction of the length, have a tendency to impair the elasticity of the steel with the result that crystallization of the material and brittleness manifest themselves. This deterioration governs the life of ropes on the Kimberley mines, all having to be rejected, not on account of wear as it is generally understood, but by the breaking of individual wires. These broken wires reveal themselves in positions which are distributed all over the length of the rope, but more particularly in that portion immediately above the skip.

This latter defect necessitates the provision initially of sufficient rope on the drum to enable short lengths to be frequently cut off immediately above the skip.

Experience would prove that the ropes have a life which is limited by the number of shocks they are able to withstand, or in other words to the number of trips the skips make. At Wesselton, Bultfontein and Dutoitspan mines about eighty trips per hour are made, although at times it has been as high as ninety-four. At these mines the skips have recently been altered so as to increase the capacity from eight to eleven tons. This alteration has made little or no diffedence to the time required for the wind. Consequently, the ropes which now hoist about 37 per cent. more ground per trip than formerly, are subject to no greater number of shocks per day. The statistical information gathered so far does not cover a period long enough to establish exactly what the results are, but it certainly does point to the fact that the life of the rope—measured by quantity of ground hauled—will be increased.

In connection with the above remarks regarding the critical speed of a rope, it might be added that from time to time mining regulations are laid down as to the proper speed of winding. It might happen that for some particular shaft the regulation speed might be at or near a critical speed. If this does happen, then a restriction—the object of which is to ensure safety—may possibly be dangerous.

At Wesselton, Bultfontein and Dutoitspan mines the engines are designed to haul from much greater depths than they are at present hauling from, and it is just possible that as greater depths are reached, flapping troubles may appear if the present speed of hoisting is adhered to.

SURFACE WEAR OF THE ROPE ON THE DRUM.

Ropes, after being in use some time, begin to show signs of wear on the outside strands. If the rope coils on the drum without any slipping, and there is no slipping on the headgear sheaves, it would be difficult to explain this wear, as it is impossible for wear to take place between two bodies unless there is a relative motion between the two. Direct slipping on the headgear sheaves, which probably exists to a small extent, might explain some of the wear, but it is only the inertia of the sheave preventing it responding immediately to change of velocity in the rope which causes this relative motion, and the author is of opinion that this is not great.

Another cause of wear between rope and drum or rope and sheave is the well-known screwing or twisting action which takes place as the rope is pulled over the surface.

It has been noticed at the Kimberley Mines that after each round trip of any skip the top of the headgear sheave carrying that skip has advanced towards the engine by a certain definite amount. This is repeated during every round trip. This motion varies in amount at the different mines, and, curiously enough, measured on the circumference of the sheave, it corresponds

exactly to the stretch given to the rope when the load is applied. This would appear to be the amount of creep expected, because when the load is hoisted a greater length is hauled over the headgear sheave than is lowered over it when the empty skip descends. This fact of stretching and contracting of a rope during hoisting and lowering respectively will explain one of the reasons why a rope wears itself on the drum of the engine. The rope is coiled on the drum in a stretched condition, and during the uncoiling or lowering period the rope leaves the drum at its circumference with less tension than it has during the winding process. This allows the rope to crawl back on the drum during the whole time of lowering; in other words, it is shortening itself on the drum during the lowering period by an amount exactly equal to the amount of stretch during the hoisting period. This can easily be demonstrated by a small model of a drum with a hoisting cord of ordinary elastic. This peculiar crawling action will also explain the excessive wear which takes place on the outer rings of a Whiting Hoist. The Kimberley Whiting Hoist was initially fitted with solid wooden rims in the wheels, but endless trouble was experienced by the wear of the wood. A section of the original rim is exhibited to show this. In a Whiting wheel this wear of course is more manifest on the wheel than on a parallel or conical drum, as the action is confined within narrow widths, but as far as the rope is concerned the wear will be the same on any hoist.

In hoists where it is necessary to have two or more coils of the rope, then this wear after the first coil is laid, takes place between rope and rope as well as between rope and drum.

Care should be taken that the groove in the sheave wheel is always of the proper radius to receive each new rope.

INTERNAL WEAR BETWEEN THE DIFFERENT ELEMENTS OF THE ROPE.

Internal wear between the different elements of the rope always manifests itself. This may be accounted for by the fact that as the rope necessarily has to bend round the sheaves and the drum, there must be relative motion between the several parts so as to accommodate themselves to the curvatures.

THE KIMBERLEY SYSTEM OF HANDLING LARGE QUANTITIES OF GROUND IN THE MINIMUM OF TIME, WITH NOTES REGARDING THE LIFE OF WIRE ROPES.

Paper Read by Mr. Alpheus F. Williams.

(*Published Vol. IX., No. 7.*)

Author's Reply to Discussion.

* * * * * * *

The author expected some criticism on the term "Factor of Safety," and that is why it is printed in inverted commas in the paper. He agrees that a higher factor of safety does not necessarily mean a higher degree of safety, and his experiments on higher tension steel ropes would appear to prove this to some extent.

* * * * * * *

The formula used in determining stress due to bending is the well-known Sederholm formula.

In reply to one question the author would say that the use of tapered ropes has not been considered.

Another questioner asked for the factor of safety at which ropes are condemned. As a rule ropes at Kimberley are condemned when they show an excessive number of broken strands, and not as a result of any test to determine the factor of safety. Recently a rope failed and yet the result of a test on a portion of the rope indicated that it was practically as good as when it was first put on.

The results of experiments on higher tensile steel ropes are submitted herewith.

The author cannot say that there is any marked difference in the life of ropes between those used on the Whiting hoist and others used on drum hoists. The number of loads hoisted, taken in conjunction with the distance hauled, would indicate that there is not much difference either way. The cost per load for 100 feet in use as indicated in the tables works out practically the same in both cases.

One critic disagrees with the remarks regarding the creep of the rope on the drum, but the author is of opinion that this does occur. On all of the Kimberley drum hoists which have plain smooth drums the tendency is for the rope to wear the drum

ıal ng Rope er 's ıte.	Break Load of per sq as p Make Certifi
	ton
	102
	102
	100
	100
	100
wn	Not kn
	123
	123
wn	Not kn
wn	Not kn
	123
	117
	123·2
	123
	132·9
	123
	123
	117
	123
	123
	112
	112
	130·57
	130·57

into a spiral, in some cases the depth of the groove worn is almost one-eighth of an inch. The rope cannot wear the drum away in this manner without at the same time wearing itself.

As to whether there is slip and wear between the rope and the headgear sheave, the author has made experiments to determine the co-efficient of friction between the rope and the sheave. This experiment was made primarily to determine the driving effect of the Kimberley Mine Whiting engine, where the rings of the sheaves on the crankshaft are tightened hard to prevent them from slipping, and where the rings in the idler pulley are left loose and act simply as guide pulleys. The experiments give definite ratios between tight and slack side of the rope when slipping takes place and also gives a uniform co-efficient of friction between rope and pulley. Knowing these particulars and also the inertia of the headgear sheave, etc., it is a simple calculation to prove whether or not it is possible for the rope to slip under the acceleration conditions which prevail in ordinary practice.

From the Journal of the South African Institution of Engineers.—Vol. X., No. 9, 1912.

NOTES ON THE MARGIN OF SAFETY REQUIRED BY GOVERNMENT FOR MAN HAULAGE AT GREAT DEPTHS.

PAPER BY MR. R. B. GREER (MEMBER.)

To be read April 20th, 1912.

At present the Transvaal Government require a margin of safety of six for man haulage. This means a margin of safety of seven when the rope is new, so that when the rope wears a little it will keep at or above the Government requirements.

Now this safety margin was generally adopted as far back as 1877, when winding at great depths was not the common practice it is now, and the reason for it was, from the experiments then tried, this margin was found to be necessary. The following is one of the tests carried out in 1877:

Weights ascertained by Dynamometer of Cage and Four Tubs of Coal at "A" Pit, Hebburn Colliery, 13th September, 1877:

	Tons.	Cwt.
Cage and full Coal Tubs (Tubs weighed on Machine)	5	1
Cage and full Coal Tubs lifted gently	5	3
Cage and full Coal Tubs as if Coal work and with 3″ slack chain	8	10
Cage and full Coal Tubs as if Coal work and with 6″ slack chain	10	10
Cage and full Coal Tubs as if Coal work and with 9″ slack chain	12	10

It will be seen from the foregoing that with nine inches of slack chain above the skip and the driver starting fairly quickly a strain was on the rope of about 2½ times the actual load.

Now what the writer contends is that for depths of over three thousand feet a slackness in the chain at the top of the cage or any other cause giving a sudden jerk would not if tested with dynamometer at the top of the shaft give anything like these surprising and alarming results, because the elasticity of the rope would take up the effects of any sudden strain; therefore a 4½ margin gives a greater margin of safety at 4,500 feet than a six margin at, say 20 feet from the shaft top, and it would be advisable for the Government to modify the present six margin of safety by a sliding scale after a depth of 2,500 or 3,000 feet.

Any of you will know that in quite a number of mines on these fields the weight of rope in the shaft is at least equal to the weight of skip and rock together.

The tendency of Mine Engineers at present is to each year require higher and higher tensile strength of steel from the manufacturers in order to be able to haul heavier loads without increasing the weight of the rope. While makers have cautiously advanced to meet the demand for good high tension steel, yet there is a grave danger in going to too high tensile strains.

At present, I should say that 145 short tons steel for wires over ·1 inch in diameter is the highest that can safely be given, as above that, no matter how good charcoal Swedish iron is used in the manufacture

of the steel, the wire has to have too much carbon in it, and is punished by being drawn to such a high pitch that there is great risk of its growing very brittle and short after a few months' wear, and never showing to those using the rope that this is the case.

The writer believes it would be much safer to lower the margin of safety in winding from great depths rather than adopt the present manner of getting over the large weight of dead rope to be carried, by going in for very high breaking strain steel. In general support of the contention of this paper, you will find that any ropes which break in a shaft, do not do so when there is a long length of rope out and the margin of safety is at its lowest, but break when the cage is nearing the top of the shaft and when the margin of safety is highest.

This paper has been read to open a discussion on this question and get the opinion of our learned and weighty men in engineering circles on the Rand concerning this important subject. It is not written after consultation with the firm of manufacturers the writer represents, but is entirely on his own responsibility.

THE MARGIN OF SAFETY REQUIRED FOR MAN HAULAGE AT GREAT DEPTHS.

PAPER BY MR. R. B. GREER.

(Published Vol. X., No. 9.)

THE CHAIRMAN (Mr. J. A. Yule) said they had to thank Mr. Greer for his paper, which undoubtedly would lead to considerable discussion.

DR. W. GLUCKSMAN (Associate Member) said that, whilst Mr. Greer's paper was a short one, inasmuch as it only dealt with one question, the point he raised was of very great importance and needed very careful consideration. If the question was dismissed in a few words there was little hope of a satisfactory answer being obtained, therefore it was essential that all details connected with the stresses and strains that ropes were subjected to should be fully examined. Only then would a final and satisfactory determination be arrived at as to whether it was or was not permissible to reduce the factor of safety. He was inclined to think that Mr. Greer was correct in his assumptions, though it was certain that objections would be lodged against lowering the factor of safety, because so many lives were dependent upon hoisting ropes, and it would be held that the proposed reduction would lead to unwarrantable risks being incurred. The fact that other Governments still maintained a factor of six would obviously be quoted. He hoped that our eminent engineers would fully discuss the matter.

On one occasion the speaker had to make calculations for a proposed hoist to serve a compound shaft 2,180 ft. vertical depth, 2,368 ft. on a 28° incline, and 284 ft. on a 35° incline. The proposed maximum hauling speed was 3,500 ft. per minute. The total pull on the rope, as a maximum, was 11 tons, taking a rope $1\frac{5}{8}$" diameter; therefore if a factor of safety of seven was adopted the breaking strength of the rope had to be 77 tons. Ropes for this ultimate load were procurable, but one was led to consider whether such strength was actually required. The greatest difficulty which the speaker met with was in determining what the stress or load would be when the cage was passing the bends. He was of opinion that the greatest stress on a rope occurred when the load was being retarded, and not during the acceleration period.

Having very carefully considered the various points he arrived at the same conclusion that the author of the paper reached, and was of opinion that the factor of safety for long ropes could be reduced without danger. As a result of his investigations he considered that the following reductions could be made:—

For depths of	Percentage Reduction.
600 feet	3%
1,000 feet	5%
1,500 feet	8%
2,000 feet	19%
3,000 feet	34%
4,000 feet	47%
4,500 feet	56%
5,000 feet	62%

Mr. J. A. VAUGHAN (Past President) said Mr. Greer had not introduced a new subject, although, personally, he would be thankful if new light could be thrown upon it. The matter was pretty exhaustively studied by the Commission appointed by the Government some six years ago, and, after correspondence and evidence had been received from engineers, not only on the Rand but from all parts of the world, the Commissioners were unable to recommend a reduction of the set factor of safety of six for the hauling of persons. It was significant to note that no lower factor than this had ever been used for the hauling of rock. As an engineer who had looked into the question, the speaker had always thought that a trial with rock hoisting was the best method of finding out whether the factor of safety now adopted for raising and lowering persons was correct. The speaker was in favour of a lower factor at great depths. He did not know whether Dr. Glucksman had read the paper which he once wrote on the subject; roughly the result he had arrived at was this:—"When speaking of the kinetic shocks created by stopping during the lowering of a load by a rope, for every one foot velocity per second created, a stress of nearly one ton per square inch was established, the stress varying directly with the velocity that was suddenly killed or created." This formula fairly well agreed, he believed, with the timeworn statistics which Mr. Greer brought out, and which he had seen in nearly every rope-maker's catalogue he had looked at. When they subjected these figures to a little analysis and imagined the engine started up suddenly in every case, and, by taking account of an increasing acceleration, found out the velocity likely to be created by these lengths of slack, and applied the rule he had given, he believed they would find this rule and the figures fairly well agreed, but, unfortunately, they never knew whether, in the tests mentioned, the dynamometer was at the top of the shaft or the bottom, or how deep the shaft was. No very great illumination had ever been thrown on the actual tests. He had never had any clear opinion as to their authenticity; he did not know whether they were ever authoritatively published in the Journal of a scientific institution. Before accepting these figures as facts they must know the conditions of the tests, and who conducted them, and under whose auspices they were published.

Mr. E. J. LASCHINGER (Past President) agreed with Mr. Vaughan that the subject was not a new one, although, at the same time, they owed Mr. Greer their thanks for having again brought the matter up for discussion. He thought the value of that discussion would lie in the fact that some new light would be thrown upon the subject, some new information gained which would take them further on the road to the solution of the winding rope problem. He considered that every engineer in the employ of the Government, or, in other words, of the public, who was concerned with the drafting of regulations for safeguarding the lives of the workers, must be very cautious. All the civilised countries of the world, when framing engineering regulations, enquired what other countries had done, and it was a safe and a sound policy to pursue. If the factor of safety was reduced and any accident happened there would undoubtedly be a great outcry and the blame would be put on those who drafted the regulations. A point which he thought weighed largely with those responsible for the regulations, and also with all engineers who had to do with the mining industry, with regard to ropes was, presuming the factor of safety was lowered, what effect would there be on the life of the rope, taking into consideration the many shocks and strains which a rope is subjected to? They knew they needed a good reserve. They could take an analogy from cam-shafts in stamp mills. For years the speed at which stamps dropped had been practically the same. As a result of experience with cam-shafts, the drop of the stamps was regulated from 86 to 98 per minute. A few years ago metallurgists and engineers thought that by increasing the number of drops to 100 they would be able to crush 4% more rock. It was soon found, by those in charge of the mills, that cam-shaft breakages in the mill increased to an alarming extent. At first it was thought it was due to the mills being old, and the breakages were considered unavoidable, but the same experience was found in new mills, and it was established that an increase in the number of drops per minute at once caused trouble in the mill. Those who had to do with electrically driven mills found that if the electric motor were speeded up trouble arose in the mill at once.

The same kind of thing had happened in regard to ropes, because a rope was not a body that hauled a load along in an even, smooth manner; it was a body which was in a continual state of intense vibration, and it had always been a great surprise to the speaker that ropes lasted as long as they did. The subject was not a new one by any means. A great deal of work had been done in connection with it in the Transvaal, and he believed almost every one there had had an opportunity of giving evidence before the Commission which sat. Until they got some new facts and new experience to go upon, he did not think they could get much further forward in the matter by reducing the factor of safety.

Mr. OSTREICHER said he had a little information with regard to static structures which he might give them. The modulus of elasticity of a bar under impact was much greater than when it was under steady stress. He had read of experiments which showed that a steel bar having a static breaking stress of 45 kilogrammes per square millimetre could stand an impact stress of 40 kilogrammes without showing any permanent elongation. This meant that the limit of elasticity rose to about double what it was under static loads. They knew that a bar ruptured by stresses due to explosions stretched about twenty times as much as a similar bar under a static rupturing load.

Mr. VAUGHAN said that, following on Mr. Ostreicher's remarks, he would like to refer to the experiments obtained from a great number of accidental breakages of ropes that had occurred here. The phenomena were precisely similar to those of an ordinary tension test—that is to say, there was just a local elongation at the point of fracture. He had seen it occur, not only at the points where the wires actually broke, but at other points. These phenomena also occurred sometimes with an ordinary static tension test. He had a fatigue testing machine at the laboratory which bent and torsioned the wire and subjected it to tension at the same time, and in this case before the wire broke it was drawn down to nearly half its original section. They could almost look upon the machine as a wire-drawing machine. Under impact, however, they would find that materials failed very similarly to what they did in an ordinary laboratory tension test. On the other hand he had found whilst testing blocks of cement under impact, the load falling on to the test piece, under conditions which would normally be expected to produce a compressive stress, that the sides of the cubes were forced apart, in fact a cone was formed at or about the point of impact, and the specimen ruptured under tensile and not under compressive stress.

THE MARGIN OF SAFETY REQUIRED FOR MAN HAULAGE AT GREAT DEPTHS.

PAPER BY MR. R. B. GREER.

(*Published Vol. X, No. 9.*)

(*Continued Discussion.*)

Mr. T. TREGASKIS said they were all very much obliged to Mr. Greer for bringing this matter up again, because the question of ropes was one they had always with them. What Mr. Greer had particularly dealt with was the winding of men from considerable depths, but he did not seem to have noticed that the regulations permitted them to wind minerals from considerable depths without laying down any factor of safety, and, before they made any representations to the Government to make any alteration in connection with the factor of safety for men, it was necessary for the mines to bring forward some information with regard to their experience of the hoisting of minerals at a lower factor of safety than six. They knew from Mr. Alpheus Williams' paper that, at some of the Kimberley mines, they started with a factor of safety for materials of less than six, and that it was rarely they had a broken rope. The Kimberley mines, however, were not very deep, so that this did not convey very much as to the factor of safety they might require in deep winding. Mr. Greer had pointed out that most of the breakages of ropes occurred, not when the skip was at the bottom of the shaft when the factor of safety was least, but when the skip was near the top, and it was quite possible that this was one of the reasons that caused the break in the rope mentioned in Mr. Williams' table. They needed to consider what this factor of safety had to cover. There was the question of shocks on the rope. He thought from the paper that

Mr. Vaughan read some years ago this point might almost be disregarded so far as great depths were concerned. It was, of course, of great importance at shallow depth. There were other points which the factor of safety had to cover. Of course there was manufacturing factor; they never could be certain that the rope was going to be of the same strength throughout. There was also a very much more serious one; that was the damage done to the rope when it was being put on, or the damage done to the rope after it was in the shaft owing to some minor accident which was not brought to the notice of those officials who should know about it. There might not be a kink, but an elbow might be put into the rope, and if so there was no doubt that the high side of that elbow became badly worn, say, for a distance of 18 to 24 inches, as it passed over the sheaves. During the inspection of the rope it was very long odds against one being able to note that particular length. The factor of safety had to cover all occurrences of that sort. If they held that a factor of safety of six was necessary for a short wind, he did not see how they could do without a factor of safety at all when they got down to the bottom of a deep shaft. In a long rope the chance was increased of getting these small minor errors in manufacture and the accidents which occurred whilst putting it into the shaft. That part of the factor of safety which was put down to cover shocks became very much less, so that he quite agreed with Mr. Greer that in a deep shaft they could do with something less than the factor laid down now, but he did not think they could cut it down very much, and before they could go to the Government and ask them to make any great changes, they would have to show them what was happening to those ropes which they were using with a factor of safety of less than six. It must not be forgotten that a broken rope in a deep shaft, whether they were hoisting minerals or hoisting men, was a very dangerous thing. It might not only rip their shaft timbering to pieces, but they might happen to be winding men in another compartment and the breakage might cause an accident in that compartment also; indeed, if a rope used in hauling rock broke in a shaft of great depth it might cause as great a loss of life as if men were actually being hoisted on it.

Mr. J. A. VAUGHAN (Member) said : As promised when this paper was read, I present a few more remarks on Mr. Greer's short paper. I am personally very pleased that Mr. Greer has again brought forward the subject of the factor of safety of ropes used for winding purposes.

I regret, however, that Mr. Greer, in bringing the subject forward, did not apparently avail himself of the data that have been presented either in Blue-Book form or in the shape of contributions to the local engineering societies. Mr. Greer practically accuses us of following blindly a standard set up in 1877, a safety standard proved to be essential owing to certain shock tests carried out at Hebburn Colliery in Great Britain at that time.

I am unaware that these tests have ever set the practice in Great Britain, and am quite sure that they have not done so on the Rand. As far as my knowledge goes, the Transvaal Mining Regulations were modelled on those existing in the German Empire, and the minimum factor of safety of six was taken therefrom.

Eleven years ago, when I started to investigate the matter, the values in general use in the catalogues of British wire rope manufacturers were: 10 for quick winding in vertical or steeply inclined shafts, 8 for quick winding on moderate gradients, and 6 for crane work.

In a paper presented by me to the South African Association of Engineers in March, 1904, it was proved that, for lengths of rope weighing less than 0.35 of the load they carried, the maximum kinetic stress at the upper cross section, due to a sudden stop in lowering, increases rapidly as the length of rope is shortened, while for a length equal in weight to (or greater than) 0.35 of the load, the kinetic stress is constant and is equal to $3\ E\ \frac{v}{V}$ when E is the modulus of elasticity of the rope, v the velocity of lowering, and V the velocity of the propagation of stress. As I stated at last meeting, this formula gives a value of somewhat under 1 ton per square inch stress for each foot per second velocity killed.

Arguing from this established theorem, I was led to propose for deep-level vertical winding the provision of a reserve of strength in the rope as contrasted with a factor of safety. This proposition comes

directly from the preceding theorem taken in conjunction with the formula for the statical stress.

If W denotes weight of cage, fittings, and contents (tons of 2,000 lbs.), L depth of winding (feet), a, sectional area of wire in the rope, then, allowing 4 lbs. per foot per square inch of wire section, the statical load on the upper cross section of the rope is equal to

$$\left(W+\frac{La}{500}\right) \text{ tons}$$

At a factor of safety of 6, the reserve of strength in the rope

$$\text{is } \left(5W+\frac{La}{100}\right) \text{ tons}$$

It will thus be seen how the reserve of strength increases with L the depth of shaft. It also increases with a, the total cross sectional area of the wires. So that we were face to face with the "compound interest" law which, as you know, depends for solution on a logarithmic equation—and the taper rope.

Quoting from a paper on kinetic shocks on winding ropes presented by the speaker in 1904:—

"Imagining for a moment that similar loads of (say) 7 tons each—consisting of 6,000 lbs. conveyance and 8,000 lbs. contents—have to be hoisted from two shafts, one of 1,500 and the other of 6,000 ft. in depth. Supposing the ropes used in these two instances are of the same grade of steel (120 tons per sq. in.), then, if they are compared at a time when they each have the minimum factor of safety of 6, it will be found that the reserve of strength in the case of the 6,000 ft. rope is approximately 2½ times that existing in the 1,500 ft. rope. The latter, when the full skip is at the bottom of the shaft, will be loaded to within (say) 45 tons of its breaking load, while the former retains a margin of about 110 tons."*

Dealing therefore with two of the most important stresses that a winding rope is subjected to, it was found that the taper rope appeared to be the panacea for statical conditions, but was not required and, as could be shown, was not the best solution when considering the shocks due to change of velocity of winding. As mentioned above, the stress due to kinetic shock, after a certain moderate depth is reached, is independent of the size of the rope or the depth of shaft. Looking at the matter practically, it is certain that the kinetic stress must reduce at depth.

Quoting again from the above-mentioned paper:—

"The following deductions appear justifiable:—

"(1) Within a certain limit of depth from the surface, this depth depending on the ratio W_s/k, a winding rope is stressed more severely than at greater depths by a kinetic shock of given magnitude.

"(2) From this limit of depth to the surface the stress for a given kinetic shock increases as the rope is decreased in length.

"(3) The portion of winding rope nearest to the load (*i.e.*, the portion that never winds on the drum) is a part that has to bear the sudden application of *every* kinetic stress due to changes in the kinetic energy of the load, and may be expected to deteriorate more rapidly than other portions of the rope.

"(4) For all depths greater than the dangerous limit the maintenance of a standing reserve of strength in the rope appears a suitable provision against the effects of kinetic shock.

"In explanation of this deduction, to which the writer imagines some importance attaches, it requires to be remembered that, although the enlarging of the rope does decrease the depth of the dangerous limit, it *does not reduce the kinetic stress* for depths beyond this limit.

"With a seven-ton load, if the rope were increased from 1.27 in. diameter to 1.6 in. diameter, the sectional area of the wires would probably be increased by 50 per cent., the weight per foot would also be increased by 50 per cent., while the depth of the dangerous limit would be reduced from 1,823 feet to 1,213 feet—that is, by one-third."*

* Extract from a paper read by Mr. J. A. Vaughan, before the South African Association of Engineers, on 30th March, 1904, entitled: "An Investigation regarding the effect of kinetic shocks on winding ropes in vertical shafts."

Mr. Greer and I are rather alone in recommending a reduction in the factor of safety or in the reserve of strength.

The Commission appointed by the Transvaal Government in the year 1905, after considering all possible evidence that could be adduced, did not advocate a reduction. Quoting from the Commission's report:—

"The Commission has carefully considered this important matter, and recommends that the present Regulation, which provides for a Factor of Safety of 6 as a minimum during the life of a rope, should be adhered to. No reliable statistics were brought forward by any witness, and none were otherwise available, dealing with the working of winding ropes at a lower Factor of Safety than 6, and the Commission is of opinion that considerable experience should be obtained with ropes working at lower Factors while hoisting mineral, before any departure could be recommended for the Transvaal practice, which has been found to be satisfactory."‡

Quoting further from the reply to the discussion on Messrs. Vaughan and Epton's paper on wire ropes, the suggestions dealing with the minimum factor of safety were as follows:—

"The authors wish to thank Messrs. Denny, not only for the full consideration they have given to the paper, but also for their offer to submit all the information with regard to the practice on the mines under their management. It is obvious that if others will do the same a mass of valuable data will be collected, which will be for the benefit of the mining industry, in that the possible defects in manufacture may be recognised, errors in treatment discovered, and a relative value attached to the many deteriorating influences, which affect the life of the winding-rope. This will all lead to a more intelligent knowledge of the actual reserve of strength existing in a rope, as well as to the necessity for its being there. The arbitrarily fixed minimum factor of safety for all cases may then become a thing of the past, and the expense incurred in securing the best conditions of working may be compensated for by the preferential treatment accorded to the best practice."§

My own opinions remain unchanged, except that I am inclined to attach even less importance to bending stresses when values of the ratio

$\frac{D}{d}$ greater than 800 are maintained, drum hoists only being considered and frequent recapping resorted to. The details of rope tests on a deep level mine where low factors were used are of interest. More such data would be very welcome. The ropes in question have had eighteen months' hard wear, rapid rock winding in a deep vertical shaft. Rope, 1¼ in. diam., 4 in. circumference. Material, 123 short tons b.s. Total load, including weight of rope, a little more than 14½ tons.

Ropes of about 80 tons aggregate breaking load, say 72 nett.
Six-monthly tests:—

6 mths.	12 mths.	18 mths.	
71·3	70·12	58·0	brittle wires.
72·3	71·3	63·0	,, ,,

So that, lately, the apparent statical factor of safety in this rope has not been much more than 4. However, the portions tested would probably only have had each to stand a load of 9¼ tons; so that their statical factor would have been over 6. These portions, however, from their brittleness, would be unsafe in anything but a vertical shaft and with steady winding. The statical factor of safety at the sheave was probably maintained. Recapping provides for safety at both ends, and the oftener this is done, within reason, the better.

Mr. W. CALDER thought that Mr. Greer's paper was a sort of despairing cry from the maker of wire ropes. Mr. Greer said: "I should say that 145 short tons steel for wires over ·1 inch in diameter is the highest that can safely be given." The whole paper seemed to point out that the point of failure of the rope was at the skip end, and that the best quality of steel was really required there, and, as Mr. Vaughan had said before,

‡ Extract from the report of a Commission appointed to enquire into the use of "Winding ropes, Safety Catches and Appliances in mine shafts," Johannesburg, Transvaal, 14th March, 1905.

§ Extracted from the reply to discussion on a paper entitled: "Wire ropes used for winding: their strength and some causes of its reduction."—The Journal of the Transvaal Institute of Mechanical Engineers, April, 1906.

it seemed to imply that a tapered rope was necessary. He did not profess to know much about the drawing of wires for ropes, but it seemed to him that if they had a varying factor of safety in the rope from the sheave to the skip, and if at the sheave they had a certain breaking stress and a certain ductility of the wire, the strength of the wire would have to vary throughout until they arrived at the skip. He did not know if it was possible for a rope maker to make a rope of this kind. Mr. Greer seemed to show that users had asked the rope makers now to make a rope which was just about the limit of the material, and that, if they wished to get a stronger rope, they really wanted a different material at the point at which the rope broke, which was usually some little distance from the skip. It had been proved that a tapered rope was not successful on these fields, and it had struck him that a rope drawn to different degrees of ductility throughout its length might prove suitable for deep winding. It was probable that their rope-maker friends could tell them whether it was possible to draw wires of this kind.

Mr. Roger Price said that, with reference to Mr. Vaughan's remarks, he understood him to say that he considered that the bending stresses in a rope depended upon the ratio of the dia. of sheave to dia. of wires in the rope, and that they did not amount to very much. He would like to ask Mr. Vaughan what he took as the value of E in this consideration. He believed that the figure had been given by Hrabak, and other eminent authorities on this subject, as twenty millions after the rope had been in use for some few weeks, he meant the modulus of elasticity of the rope as a whole and not of the wires.

Mr. Vaughan said he was pleased that Mr. Price had put this question because he believed he had left out one reservation in his statement. He ought to have said he was only talking about drum hoists and not those other types that resembled rope-breaking machines. He considered one thousand to one a very good ratio of diameter of sheave to diameter of wires in rope. He thought he made it clear that the formula

$E\frac{d}{D}$ was not applicable in the case of the wires in a wire rope. He had only alluded to the ratio $\frac{D}{d}$. He only mentioned E when quoting the formula $E\frac{v}{V}$ which gave the kinetic stress in a rope.

Mr. Schmitt said the remarks made by Mr. Vaughan brought to his mind one or two points. The two examples given that evening as representative of Rand practice could be largely supplemented if the engineers on the mines could be induced to give them some of the information they were known to have. All the information they required was available on the Rand if they could only get at it. It was a splendid opportunity for engineers to come forward and tell them what they knew about ropes. Another point was that Mr. Vaughan had promised a paper on the subject and he hoped, as a member of the Editorial Committee, that it would come very soon. Mr. Vaughan had read some extracts from his previous paper presented to the South African Association of Engineers, and he advised those who were not members of the South African Association to get the printed volumes of that society, as they contained a great deal of useful and interesting matter, particularly in connection with the subject under discussion.

Mr. Kenneth Austin said it was a long time since they had had the memorable discussion on "Winding From Great Depths." Many wire rope makers took part in that discussion, and they put forward the idea of making a rope with various grades of hardness. Things had improved a great deal since then in many directions, but he did not know whether Mr. Greer could inform them whether the wire rope manufacturers had improved with the times—in that direction he meant.

Mr. Greer said he did not think that manufacturers were able to draw a wire of varying tensile strength and ductility.

THE MARGIN OF SAFETY REQUIRED FOR MAN HAULAGE AT GREAT DEPTHS.

Paper by Mr. R. B. Greer.

(Contributed).

By Dr. Glucksman.

Mr. J. A. Vaughan was good enough to forward to me his very lucid paper, entitled "An Investigation Regarding the

Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts," read at the March meeting, 1904, before the South African Association of Engineers, as bearing on my remarks made on Mr. Greer's paper, in April last.

Although Mr. Vaughan's investigation differs from mine, I am pleased to note that some of Mr. Vaughan's conclusions coincide with mine, and Mr. Vaughan in his paper corroborates Mr. Greer's suggestion, namely, the deeper the shaft is the smaller the " factor of safety " can be.

I think that Mr. Vaughan has answered the questions put in Mr. Alpheus Williams' paper, and which ought also to be put in Mr. Greer's paper, viz., " What is the ' safety factor ' "? Invariably people are mistaken in thinking that a rope with a safety factor of, say, 10, is twice as strong as a rope with a safety factor of only 5. It may be that the difference is far more than or it may be that it is far less than twice; all depends mainly upon the depth of winding. " Safety factor " by itself can hardly be defined, as it has probably—I should say surely—little meaning. It is far better, it ought even to be compulsory, to replace these old, and somewhat meaningless, words by a new term, such as brought forward by Mr. Vaughan, viz.: " Reserve of strength." Each rope ought to have a reserve of strength of, say, about 100 tons, no matter what the hoisting depth, or kind and form of make, or material the rope is made of, or other conditions, such as fatigue due to incurvations, when passing bends, sheaves or drums, are. In this case we would have an uniformly strong rope everywhere, or what I would call " a rope of equal resistance."

Another point, which is probably old, and which everyone who has to deal with this question repeatedly discovers is that the rope ought to have a variable section when used for winding from great depths. I have been told that this cannot be brought forward on these fields, as the system was done away with, locally, some years ago, on account of some unfortunate accidents which happened on the Rand, and the fault was found to lie with the tapered rope.

To accuse is easy, but to excuse is difficult. If accidents have really happened I don't think the fault was with the principle of the tapered rope, as I have not heard of anyone publicly saying so. On the Continent the type is in great vogue; in some countries it is the only type used—for instance, in Belgium, probably because the inventor (Vertongen) was a Belgian.

Mr. Laschinger's statement, that, as we ignore what happens, and what kind of stresses occur, with a moving rope, it is more advisable to give a large margin of safety, is correct, but only to a certain extent. Practically every piece of mechanism has some hidden secrets, which are, so to say, not solvable, but with the amount of data available, helped out by sound reasoning, we venture to give one or other solution to every question which crops up. Now Mr. Laschinger is afraid that when diminishing the safety factor, though there may be no immediate danger of the rope breaking, we may shorten the life of the rope. That may or may not be so. We have neither affirmative nor negative proof. I had under my charge in Spain a rope which I had doubts about, and, after having examined it, I estimated that its safety factor was about 2. I advised the immediate removal of the rope, but encountered great opposition. The old hands on the property told me that the rope had been working for over four years, and would undoubtedly last another four years. I had to give way, and, after twelve months' continuous work the rope was again examined by me and my successor, and the safety factor, by test, had not varied at all; it remained as it was after my first examination. In this particular case the very low safety factor in no way affected the life of the rope. I can but repeat that " high " or " low " safety factor is only a form of speech, with little or no meaning in it, and that it is " the reserve of strength " which is important.

One of the most dangerous kinds of shock is that which in French is called *coup de fouet* (" click of a whip "); it occurs always when lifting the load, just at the start. Those interested in this particular shock I refer to Mr. Vertongen's paper in *Bulletin de la Société de l'industrie minérale de Saint-Etienne, 2nd series, b xiii, 2nd edition.* Further there are shocks due to the instantaneous action of the brake, and also to a sharp change of direction in the shaft.

If there are several layers of rope on the drum, every subsequent layer produces a secondary fatigue on the layers underneath.

But all these fatigues, tensions and stresses can be figured out in each individual

case, and all we have to do is to add, after all calculations are made (say) 100 tons as a reserve of strength. And it is very likely that a safety factor of two in one case will be better than a safety factor of 7 in another.

From the "Journal of the South African Institution of Engineers," No. XI., No. 10, 1913.

Mr. KESTNER then read his paper:—

HAULING FROM GREAT DEPTHS.

In every detail of the mining industry except hoisting important changes have been made in recent years. The hoisting machinery has remained practically unchanged, *i.e.*, the old time-honoured winch-hoist is used. With increasing depth the weight of the ropes naturally increases considerably, and it follows that the masses to be accelerated or retarded increase in proportion. The object of this paper is to investigate the manner in which the efficiency of the hoisting machinery can be improved, and the conditions obtaining when hoisting from 3,000 or 4,500 feet depth, and when hoisting with steam or electric-driven machinery. With a fixed daily output to be handled, it should firstly be determined whether it will be more advantageous to hoist lighter loads quicker or heavier loads slower. Before, however, calculations regarding this can be made, we have—as stated before—to first consider the hoisting rope as, at great depths, the whole machinery will be influenced by its dimensions.

Regarding the degree of safety in a rope and its flexibility one can only arrive at proper deductions if the mines keep statistics of the ropes used, because the influence of acid mine water on the rope and also the jumping of the rope during the journey cannot be calculated. It is, therefore, absolutely necessary when speaking about hoisting from great depths to give special attention to the rope question, and in the following I will endeavour to put this most important question more clearly before members.

Regarding the weight of load to be hoisted (which factor has, conjointly with the depth, the greatest influence on the strength and strain of the rope), I consider it necessary that such loads when hauling from great depths should not be decreased; they should rather be increased up to the rope's safety limit in order to make up for the time lost in accomplishing the longer journey. It is obvious that the greater pull on the hoisting rope caused by the greater depth and bigger loads has a great influence on the rope diameter, and therefore, also on its weight. It is evident that when using heavy ropes the handling of hoisting machinery is made more difficult and even great dangers to hoisting may arise. The immediate result of the higher rope weight is increased static pressures on the hoisting engine and headgear sheaves, and thereby involves a strengthening of the whole plant. Of importance also are the dynamic forces which arise when hoisting from great depths, and the rope weight shows clearly its influence on the hoist. This influence is especially great in the case of hoists with rope drums, the size of these drums, and therefore their weight, increases with greater depths and greater weight of ropes, and such increase on rope drums is in a greater proportion than on the so-called Koepe sheave, and I hope that our engineers when deciding on hoisting plants for greater depths will in future also take into consideration this type of hoist. I have specially considered this Koepe hoist in the comparison of the steam and electric winder.

Reverting back to the ropes, I wish to state that at home excellent experience has been made with ropes made of material having a breaking strength of 100 to 120 long tons, and I believe that the mines on the Witwatersrand must seriously consider the use of ropes of even higher breaking strength of material than this. It will be in the interest of the mines to use ropes lasting, with high-speed hoisting, about two years, and it will be generally sufficient when ordering new ropes to state that they must have at least a factor of safety 50 per cent. above the Government requirements, *i.e.*, that the rope has at least a factor of safety of 9. As the necessary calculations would draw out this paper to an enormous length I must abstain from making an attempt to determine the economy of using ropes constructed of higher breaking strength material.

The question of the increase of rope cross section when hoisting from deep levels has been already touched upon several times in the technical press as well as in this Institution, and it has always been especially pointed out that the cross sections of the

rope do not increase in direct ratio to the depths. It might, therefore, not be out of place to show by diagram the manner in which the cross sections of the ropes increase. The diagrams (Figs. 1, 2 and 3) are based on the following formulae for calculating the rope cross sections. Taking the cross section in square meters as " S "; the permissible tensile strain in kg square meter as " t "; the hoisting load in kg as " Q"; the depth of the shaft in meter as " T "; and the weight of a cubic meter of rope in kg as " g." The highest stress on the rope can be found by:—

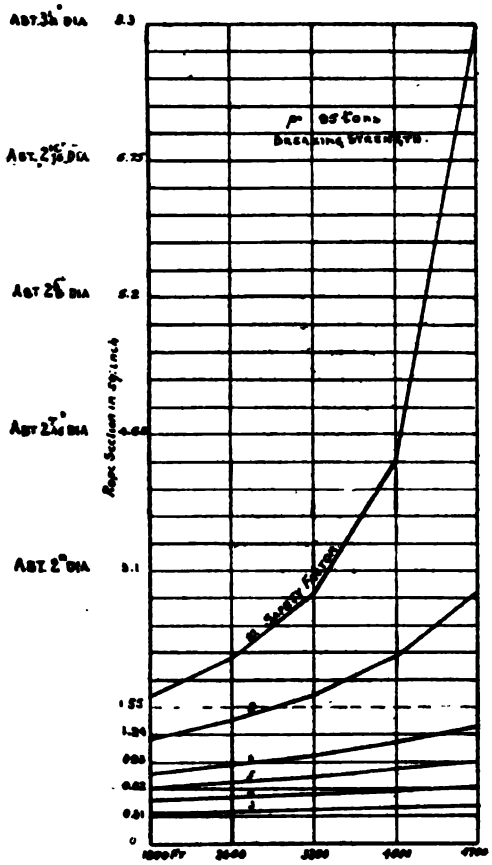


FIG. 1.

$$S. \times t = Q + (g \times S \times T)$$

hence we gain for the cross section of the rope and therefore also for the rope weight:

$$S = \frac{Q}{t - (g \times T)}$$

On this simple but most important formula depends a great deal for the future of our shaft ropes, and, therefore, it requires a good deal of attention, and I wish to call members' attention to the three diagrams which give details with regard to the increase of the rope cross section with increased depth. The diagrams are based on high safety factors.

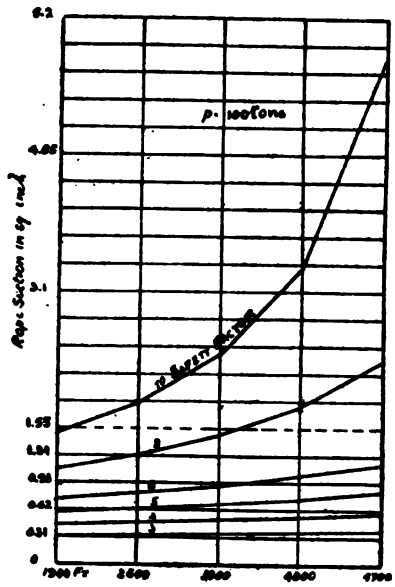


FIG. 2.

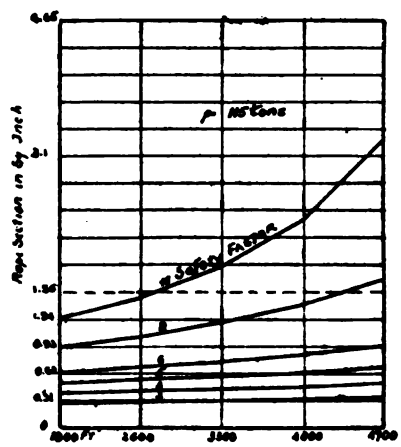


FIG. 3.

With regard to the safety factor in ropes I wish to refer to the Transvaal Rope Commission of 1906, wherein several engineers advocated a strength margin instead of a safety factor.

It is evident that I had to base the calculations for my curves on definite depths, and I have selected 3,300 ft. and 4,000 and 5,000 ft. There is no shaft on the Rand as yet sunk to the latter depth, but I expect it will not be very long before hoisting from 5,000 ft. depth will be an accomplished fact.

* * * * * * *

Reverting back to the use of rope having a high factor of safety, I believe that there should be a limit to such safety factor, and that a safety factor of 7 is quite high enough because with it the dimensions of the rope are too great. It is, however, also advisable to take ropes having a very high breaking strength, as stated in the beginning of my paper.

It is further of great advantage for hoisting if the weight of the rope is balanced by means of a tail rope. The saving in power, *i.e.*, steam and energy consumption, are great, amounting to about 20 per cent.

Mr. J. A. VAUGHAN (Past President) said it was a question whether they should say anything whatever about this paper that night, the author not being present nor the diagrams which he referred to being on view. However, as Mr. Kestner had to say something more and produce diagrams, there were one or two points which it might be well to remind him of. Taking the factor of safety question into consideration, it appeared to him that Mr. Kestner was not consistent in his paper. He started off by advising a factor of safety of 9, and at the end he reduced the factor of safety to 7. As far as he (Mr. Vaughan) could see, this reduction was made on account of the Koepe hoist, and therefore did not appear justified, because the kinetic conditions might be more severe, seeing that the single sheave used in this system had less inertia than the massive drums of other hoists. He did not propose to proceed with a criticism of the Koepe hoist as applied to winding from the various levels, which even in their deep level mines had to be undertaken. They knew there would be difficulties about that. He would like to say a word about the formulae which Mr. Kestner had put forward. Of course, this was not the first time that a formula had been brought forward and placed in their journal with regard to the size of ropes required to deal with different winding conditions. He thought that their Editorial Committee might have Mr. Kestner's formulae translated into the usual units in order to facilitate their consideration. He thought that Mr. Kestner would have been well advised in reading a paper, which, after all, was not on a new subject, if he did not agree with the formulae previously given to criticise them and show how they differed from his own. In listening for the first time to a paper like this, it was impossible to immediately appreciate the attitude taken up by the author, and he was unable at the present time to criticise the formulae brought forward. Mr. Kestner had referred to the acceleration and retardation stresses that occurred on the rope. Of course, they had in Mr. Alpheus Williams' paper an endeavour to show, from records automatically taken from the driving shaft of a winding engine, the changes in speed which were occurring. It occurred to him lately that it would be practically possible to measure these at the same time in the conveyance. When he was engaged on the Safety Catch Commission, he brought forward an instrument there which for the purpose of measuring shocks was quite good enough. They could measure fairly correctly the sudden change of velocity which occurred when the safety catch came into action. This rough instrument could be improved upon, and when they considered the precision attained with other scientific instruments he thought it was practically possible to have an instrument (perhaps something after the style of an aneroid barometer) which could measure and record the changes of velocity which occurred in a descending or ascending conveyance. He was not a sufficiently good inventor, nor had he sufficient time, to bring one forward, but he thought it was a proposition which was capable of solution. Of course, as they knew, the change of velocity had been measured in a locomotive, and it could be done in the conveyance in the shaft, and they would then have a check on an arrangement such as Mr. Alpheus Williams had brought forward to record the changes at the engine. They would remember that Mr. Williams' device was criticised by one or two members as not applying exactly to the conditions which existed at the other end of the rope, so he thought that it would be a good thing to endeavour to record the retardations or accelerations simultaneously at each end of the rope. He wished to thank Mr. Kestner for his paper, and he hoped his remarks would be taken as nothing more than an *en passant* criticism. There was one other

point, the apparent neglect on the part of Mr. Kestner of the material at his disposal in Mr. Greer's paper, and in the discussion that ensued on it. Mr. Kestner advised the use of ropes made of high breaking strength materials from 110 to 120 long tons, which would mean from 123 to 134 short tons. They had gone beyond that already on these fields, and he thought that fact should be brought to Mr. Kestner's notice at the earliest possible moment; for this reason, therefore, he mentioned it.

HAULING FROM GREAT DEPTHS.

PAPER BY H. KESTNER.

(Published in Vol. XI., No. 10.)
(*Continued Discussion.*)

Mr. ROGER PRICE (Member) said:—In the opening paragraph, the author stated: "The object of this paper is to investigate the manner in which the efficiency of hoisting machinery can be improved, and the conditions obtaining when hoisting from 3,000 to 4,500 feet depth, and when hoisting with steam or electric-driven machinery." The speaker had carefully perused Mr. Kestner's paper, and felt bound to remark that the author had thrown no new light on a subject which for many years had engaged the close attention of some of the best engineers in the world.

Mr. Vaughan had already dealt with the author's remarks on the breaking strength of rope material and on factors of safety. The formula given, on page 268, for obtaining the size of rope required for a given service appeared to be vague and misleading. "S" was stated to mean the cross section in square metres. It was evident from the formula that if "t" was the actual tensile stress, "S" must be the "useful" cross sectional area of wire in the rope. But, if this was so, and if "g" was the weight per cubic metre of rope, then "g S T" could not be the real weight of T metres of rope.

"Q" was given as the hoisting load. Although the formula indicated that "Q" did not include the weight of the suspended rope, it would have been well to state clearly that "Q" was the weight of the vehicle (skip or cage) attached to the rope, plus its contents.

The use of "g" (weight per cubic metre of rope) appeared to be unnecessarily cumbersome. Again, it would have been better to use some other symbol than "T" for the depth of the shaft (though its origin was quite obvious).

This criticism might appear censorious, but it was surely an axiom that, in propounding any formula, it was well to state the premises very clearly, and to choose symbols which obviated ambiguity.

The speaker took the opportunity to publish a "Rope" formula which he had used for many years, and which, to the best of his knowledge, was due to Mr. Hans C. Behr.

Let X = The "useful" cross section of steel in the rope in square inches.
,, f = The safe fibre stress to which the steel was to be subjected in lbs. per sq. in.
,, W = The weight of the vehicle attached to the rope, plus its contents, in pounds.
,, d = The nominal diameter of the rope in inches.
,, L = The length of suspended rope in feet.

It was known that, for ordinary 6 strand ropes of the usual constructions (from 7 to 19 wires per strand) $X = \cdot 34d^2$ approximately (neglecting the single core wire of each strand, and making the usual average deductions for laying up). It was also known that the average weight of such a rope in which X = 1 sq. in. was 4·82 lbs. per foot. The weight per foot of a 1 in. rope (in which X = ·34 sq. in.) was ·34 × 4·82 = 1·64 lbs. approximately.

Let θ = the inclination of the shaft.

Then:

$$fX = \sin\theta\,(W + 4{\cdot}82\,LX) \qquad (1)$$

$$\text{and } X = \frac{W \sin\theta}{(f - 4{\cdot}82\,L\sin\theta)}. \qquad (2)$$

For a vertical shaft $\sin\theta = 1$

$$\text{and } X = \frac{W}{(f - 4{\cdot}82\,L)} \qquad (3)$$

Substituting $\cdot 34d^2$ for X in (2) and (3).

Then:

$$d = \sqrt{\frac{W \sin\theta}{\cdot 34\,(f - 4{\cdot}82\,L\sin\theta)}} \qquad (4)$$

or for a vertical shaft:

$$d = \sqrt{\frac{W}{\cdot 34\,(f - 4\cdot 82\,L)}} \qquad (5)$$

Example:

Let W = 20,000 lbs.
,, f = 40,000 lbs. per sq. in.
,, L = 4,000 ft.
,, θ = 30° (sin θ = ·5).

Then:

$$d = \sqrt{\frac{20{,}000 \times \cdot 5}{\cdot 34\,(40{,}000 - 4\cdot 82 \times 4{,}000 \times \cdot 5)}}$$

$$= \sqrt{\frac{1}{1\cdot 032}} = \cdot 984'', \text{ say } 1''$$

and for a vertical shaft:

$$d = 1\cdot 685'', \text{ say } 1\tfrac{11}{16}''.$$

Except with regard to the ambiguity of the symbols " g," " S " and " t," Mr. Kestner's formula was identical with the formulae (2) and (3) given above. The speaker, however, preferred formulae (4) and (5) because they solved for " d " at once.

Further, since Mr. Kestner did not establish the relation between " g " and " S " (even though that relation might be only approximate), it followed that in his formula both these quantities were unknown.

The speaker confessed to his complete failure, after some hours' work, to understand Mr. Kestner's Figs. 1, 2 and 3.

From Fig 1 it was evident that the values given for " Rope sections in sq. ins." were (approximately) the " solid " cross sectional areas of the various ropes. It was incredible that the author took these values as the " useful " cross sections, but, as already remarked, in that case what happened to the formula, unless, again, spurious values of "t " were chosen?

In Fig. 1 it would be observed that the " Rope sections," marked 5·2, 6·75 and 8·3 should be 6·2, 7·75 and 9·3 respectively, and, therefore, presumably, the upper part of the 10 F.S. curve must have been incorrectly plotted.

In Fig. 2 the first error again appeared. In Fig. 3 this offending numeral had happily disappeared. The speaker supposed that the values p = 85, 100, and 115 tons appearing in the diagrams were the ultimate breaking strengths of the steel in each case. Assuming this to be so, he examined the " 10 F.S." curve in Fig. 1, and found that at 1,900 feet depth the rope size given (1.7 square inches) corresponded to a diameter of 1.472 inches. Using the formula $X = \cdot 34d^2$, it would have a breaking strain of 63 tons, or a safe load of 6.3 tons, of which amount 2.9 tons would be available for " W " (or " Q ").

Assuming this load (2.9) tons to be suspended at a depth of 2,600 feet, " d " would be 1.95 inches diameter, with a breaking strain of 110 tons, for a total load of 11.0 tons.

At 3,300 feet, these values become:— " d " = 4·125 inches, breaking strain 490 tons for a total load of 49·0 tons.

And at 3,526 feet " d " became infinity.

After plotting the " solid " cross sections corresponding to these values of " d," as far as possible, on Fig. 1, the speaker decided to abandon further investigations, as he had evidently failed to grasp the meaning of the diagrams.

Mr. A. S. Ostreicher said the object of the following study was not so much a criticism of Mr. Kestner's rope formula, as an investigation how far the present formulae express impersonal verifiable facts concerning ropes. And at the outset he felt bold enough to state that none of the formulae he had seen could claim to be rational. In the best case they might be rough approximations which, under certain conditions, would give results in accordance with experience, not because they were reliable, but because they were prescribed by law.

A formula was a general statement of the interrelation of facts or things in the symbolic language of mathematics. Nothing more nor less. The language in which a statement was made, did not affect the facts; one could always translate them or have them translated into everyday language. If this would be borne in mind much over or underestimation of mathematics would cease.

What did the formulae for ropes state? Simply: " if a rope is statically loaded by so many pounds it is stressed so many lbs. per sq. in." Did this describe the essential facts on which the life of a rope depended? Certainly not, even when stating that the rope must have a certain factor of safety. In this respect Mr. Alpheus Williams' tables would enlighten members if they

analysed his figures. Low stressed ropes had not always the longer life, many failed sooner than less " safe " ropes. The factor of safety was therefore no criterion of the life of ropes. " What is it then?" was the question which could only be answered by investigating the conditions on which the working stress depended, and after that it remained to find a working hypothesis—*i.e.*, a thought model of the behaviour of ropes.

Everyone knew that the working stresses in permanent structures were considerably smaller than in temporary ones, that the unit stress of a connecting rod was much lower than that of a spring, and that the working stress of a rope was lower than that of a spring, or to put it clearer, a spring might be stressed nearly to the elastic limit of the material for the statical load, whereas the rope must be stressed considerably lower, if its life was to equal that of the spring. The reasons for this were readily found, when they considered the phenomena, not as the encyclopaedists did, as a play of forces, but as modern physicists did, considering configuration and energy, *i.e.*, the capacity of the material for work. Since the publication of the classical experiments of Prof. Bauschinger they knew, that no known material was elastic in accordance with the definition of physicists, *i.e.*, the stresses were not simple functions of strains. Time, and the technological state of material influenced the stresses. Permament sets occurred under relatively low loads, and it seemed that there was an asymptotic law for the disappearance of deformations in regard to time. Not only the overstressed bar showed permanent sets, but each bar tested showed them, when loaded and unloaded. That these permanent sets were so minute, that they could for most practical purposes be neglected did not alter the fact. He thought that it was Bauschinger who proved that the yield point could be raised by overstraining, that by it the elastic properties as defined by Hooke's law were temporarily upset, and the opposite elastic limit lowered. The same experimentist distinguished between an artificial and a natural limit of elasticity, and modern experimentalists asserted that there was a flow of material to the weakest spots; some proved even that gliding surfaces were inaugurated long before the elastic limit was reached. Due to drawing of wires and due to spinning of the ropes the outer surface of the wire was hardened, the stresses unequally distributed, and the material could not so readily flow to the weak spots as in tempered springs. This was the explanation.

Fatigue experiments nad demonstrated that the material deteriorated when the stress range reached a certain limit, *i.e.*, the capacity for work diminished due to the cumulative effects of the minute permanent sets. This was a well known fact, but not acknowledged in the text books. Ropes broke when not removed before the capacity for work was exhausted; the chains of cranes must be annealed; the same applied to thousands of parts of machinery, and it was stated that the segments of the early cast iron arch bridges over the Severn became in the course of time so brittle that a light shock, like a tap of a hammer broke them into fragments. Some years ago, Mr. Vaughan showed the speaker some samples of old rope wires which had entirely lost all ductility; they broke when slightly bent. From the foregoing it was evident that the capacity for work diminished when the material was alternately loaded and unloaded, and there was no doubt that the strain-stress diagram of a test piece measured the capacity for work of that piece. Professor Unwin, Prof. Tetmejer, Prof. Inzi, and others, maintained that the work done during testing up to the point when the maximum stress was reached was the capacity for work, provided testing took place slowly; see Fig. 1, in which the portion not shaded in the strain stress diagram represented the work done, due to drawing out the material locally. If this assumption was correct, the margin of safety could be defined quite naturally.

If a rope was loaded gradually, work was done, and by adding further loads the rope would give a stress strain diagram, as shown in Fig. 1. The additional load traced out the portion shaded horizontally, and the load proper, the trapezium, shaded vertically when the test piece broke. If the load was applied suddenly, the work done by the load during its action up to the breaking point was represented by the rectangle, *i.e.*, by the area of the trapezium and the area of the cross hatched triangle. Since in a wire rope a sudden application of load was possible, and the triangle was relatively small, he defined the margin of safety of a rope as the horizontally shaded portion of

the strain-stress diagram, as the excess over the stress-elongation work (including the bending stresses). In mathematical symbols,

$$ft = lw\ (k - pe) \text{ foot lbs.},$$

where " l " is the length of the rope in feet, " w " the area of cross section of the wires in sq. in., " k " the capacity for

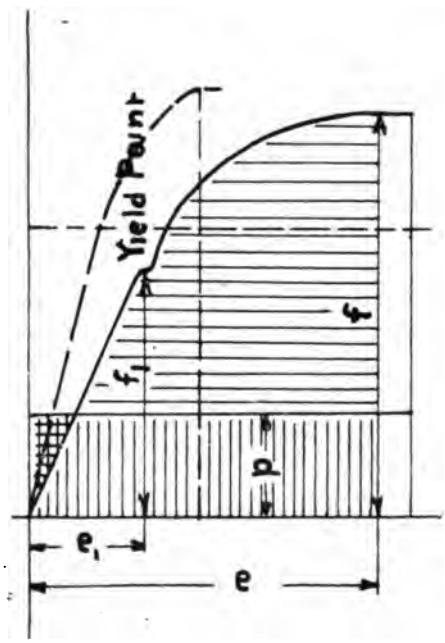


FIG. 1.

work of one cubic inch of rope in inch lbs., " p " the statical tensile stress, " e " the elongation of the rope.

Experience showed that ropes stretched, *i.e.*, permanent sets occurred in spite of loads below the elastic limit, and that the material of the rope deteriorated. He ascribed the cause of this stretching of the rope to an indefinite number of indefinitely small shocks, and the object of the margin of safety was to compensate for the cumulative effect of these shocks. Further, the cumulative effect of shocks was measured by the extension or stretch of the rope.

Since no data about the capacity for work of wire ropes were available, he approximated them from data which he had received by the kindness of Mr. Vaughan:

New Ropes.

Plough St. 134·2 t/sq. in. 2·13 lbs. per foot, length of test piece 53·75 inches.
Yield point 35·3 t, extension ·8 in.
Breaking load 58·2 t, extension 2·42 in.

Extra Plough St. 131·6 t/sq. in., 3·37 lbs. per ft., length 68 in.
Yield point 60·8 t, extension 1·02 in.
Breaking load 106·5 t, extension 3·67 in.

Old Ropes.

Extra improved Pl. St. 138 t/sq. in., 2·13 lbs. per foot, length 53·75 in.
Yield point 40·2 t, extension 1·02 in.
Breaking load 58·4 t, extension 1·20 in.

Special Pl. St. 134·2 t/sq. in., 4·268 lbs. per foot, length 52·75 in.
Yield point 40·2 t, extension 1·48 in.
Breaking load 117 t, extension 2·43 in.

The cross sections of wires were not given; he estimated them by dividing the weight of the wire in lbs. per foot by 4. Since the calculated cross section was a rough approximation, it sufficed to regard the curve as a parabola. There was no doubt that by planimetering the automatic diagrams a greater exactness could be obtained, provided the area of the cross section of the wires had been known. But, as things stood, he did not feel justified in introducing unreliable figures. The table showed the figures.

The next step was the determination of the cumulative effect of shocks which was measured by the loss of kinetic energy of the hoisted mass, viz., by

$$m\frac{v_1^2 - v_2^2}{2} = L,$$

where $v_1 - v_2$ is the speed variation. If v denotes the maximum hoisting speed, we may write.

$$L = ft.\ \frac{m\ v^2}{2}$$

Thanks to Mr. Williams records, published in the Institution's transactions, the cumulative effect of the shocks could be estimated.

The capacity for work must be equal to the work done in the strain-stress diagram

+ the effect of shocks, *i.e.*,

$$lw\,(k - pe) = x\frac{w}{g}\frac{v^2}{2} \text{ foot lbs.}$$

and

$$x = \frac{lw\,(k - pe)}{\frac{w}{g}\frac{v^2}{2}}$$

He took at random the rope No. 29 at the Kimberley shaft:

Length of hoisting rope 2520 + 80 − 1970 = 630 ft.

the speed variations did not pass the sheaves. Thus 10×916 lbs. must be deducted from the total pull given there.

Pull: $P = 63470 - 9160 = 54300$ lbs.

working stress,

$$p = \frac{54300}{1{\cdot}04} = 52200 \text{ lbs./sq. in.}$$

The ropes at Kimberley were soft, 112 tons /sq. in., hence he was not far from the truth in assuming a capacity for work $K = 8000$ in. lbs. and an extension of ·06.

CAPACITY FOR WORK OF WIRE ROPES.

Breaking Strength of Wire t/sq. in.	Breaking Strength of Wire f t/sq. in.	Yield Point Wire f_1 t/sq. in.	Elongation to f Rope e	Elongation to f_1 Rope e_1	$z = e - e_1$	$\frac{1}{2} f_1 e_1$	$\frac{1}{2}(2f + f_1)$	$\frac{K}{2.000}$ inch t.	Capacity for Work K. in.lbs.	Quality of Rope.
										New.
134·2	105	64	·0453	·0149	·0304	·48	2·77	3·25	6,500	Plough.
131·6	107	68	·0541	·0149	·0392	·51	3·68	4·19	8,380	Extra Plough Old.
134·4	117	84	·0461	·0281	·0180	1·18	1·91	3·09	6,180	Extra Imp. Plough
138	109	76	·0221	·0108	·0166	·41	1·41	1·55	3.110	Spec. Plough

Cross section of rope $\frac{4.16}{4} = 1.04$ sq. in.

Mass to be hoisted:

$$\frac{\text{lbs.}}{g} = \frac{29305}{32} = 916$$

In the table an acceleration of 10 ft./sec.^{2} was introduced. His investigations of the diagram of the Kimberley shaft showed much smaller accelerations, and as the diagram which he received from Mr. Williams referred to the winding sheaves, not to the hoisting rope, he presumed that the sag of the inclined rope acted like a spring, so that

$$x = \frac{630 \times 1{\cdot}04\,(8000 - {\cdot}06 \times 52200)}{\frac{81^2}{2} \times 916} = \frac{655\,(8000 - 3132)}{3005000}$$

$$= \frac{3188540}{3005000} = 1{\cdot}06 \qquad \mu = 3188540 \text{ foot lbs.}$$

That is: the cumulative effect of impact could not much exceed the kinetic energy of the mass to be hoisted.

The test of a hypothesis lies in its application to facts. His hypothesis was that the margin of safety was expended in compensating the cumulative effects of shocks,

and that shocks caused extension of the rope. If the ropes at the Kimberley shaft were all equal, and the ratio capacity of work to stress work p e was a constant, and the effects of shocks would cause approximately equal stretchings (which was probable, because hard wires stretched less than soft ones), then the number of loads hoisted per foot stretched should not deviate much either from each other, or from the mean.

For the ropes at the Kimberley shaft there was:

No. of Rope ...	25	26	27	28	29	30
1000 of Loads Hoisted ...	298	351	336	443	316	427
Stretch of Rope	37′	34′	36′	40′	35′	55′
1000 Loads Stretch ...	807	1020	933	1110	903	778
Mean: 930 Loads per Foot Stretch.						
Factor of Safety.	7·0	7·0	7·5	7·0	7·9	6·3

Bearing in mind that the ropes differed in their properties, and were not removed in the same states, the above figures confirmed his views. He confessed that other ropes which he examined did not agree so well; there were some exceptionally good and some exceptionally bad ropes. But in general the loads hoisted per foot of stretch tended towards an average value, and the exceptions could be rationally explained, (1) by the personal factor, (2) by the quality of the rope, (3) by the recovery of elastic properties after long rest.

In his investigations the recovery of elastic properties of a rope had not been accounted for, for the simple reason that they knew practically nothing about it, and what they did not know did not exist for them, for the time being, or, as a scientist would say: "We can only consider verifiable facts."

He was well aware of the imperfections of his hypothesis; it did not exhaust the subject, and was given as an attempt to bring the calculation of ropes on to a more rational basis than that used up to the present. A great number of experiments and observations were required to exhaust the subject, which was economically and ethically of the greatest importance. From the greater number of standpoints an object was viewed the clearer were the conceptions obtained of it, and this might excuse him putting his views forward.

From the "Journal of the South African Institution of Engineers." Vol. XIV., No. 2, 1916.

NOTE ON THE VALUE OF ANNEALING THE CONNECTING ATTACHMENTS ON WINDING PLANTS.

By J. A. Vaughan (Past President).

On 25th April last, in one of the incline shafts—25°—of the Eastern Rand, a timber trailer broke loose from a skip and dashed to the bottom of the shaft where natives were working, six deaths resulting.

The trailer weighed 2,800 lbs., and was connected to the skip by two short 1¼ in. chains on to a 1⅜ in. ring, 6 in. internal diameter, on to a 1¼ in. shackle, on to a steel wire sling 9 ft. long and 1 7-16 in. diameter with a thimble at each end.

The driver was lowering the load when some small obstruction in the shaft probably held up the trailer. The descending skip gave it a heavy blow which, with a drop of 9 or 10 feet of slack of sling, etc., ruptured the ring and set the trailer free. The ring apparently broke first through the welded side, and then, opening out, fractured on the opposite side. At the fractures there are no signs of any flaw.

The ring had been in use about two years, and there was no record of it ever having been annealed.

The sketch (Fig. 1) shows the ring and its dimensions. The first break occurred at point C, through a weld, but not to any great extent along the welded surfaces. The material here was somewhat swollen and measured 1½ in. bare. The ring, after fracturing at point C, opened out, and then a second fracture occurred at point A, leaving the ring in two halves. The fractures at

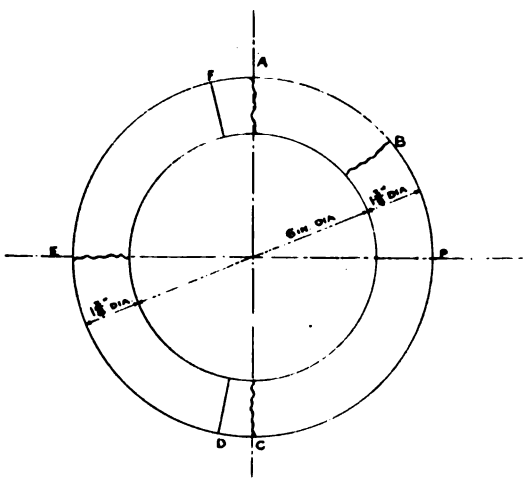


FIG. 1.

A and C may generally be described as coarse crystalline, a small portion of the welded surface showing up in C (Fig. 2), and same fibrous bands in both A and C—more in the latter. The left-hand portion of the ring was sawn through at F and D, the portion F E D was straightened, and the portion A B C was annealed.

The straightening process of the piece F E D was effected by heating to barely a red heat and bending without hammering. The annealing of the piece A B C was attempted by heating it to a bright red heat and leaving it to cool in red-hot ashes for twenty-four hours. This may not be an ideal method of annealing, and probably some experience of better heat-treatment processes may be elicited in the remarks of other members in dealing with these notes.

The certificates of the tests of the annealed and unannealed portions conducted in the Mechanical Laboratory of the Mines Department (see Table), show that the ductility was greatly increased. Had this ductility been present in the ring, it is possible that the accident might have been prevented.

The actual plastic yielding would not in itself have been sufficient, but the ring might have elongated to oval shape, and the yieldingness of the other parts of the connection would also have been of service in reducing the maximum stress according to the Principle of Work.

If the trailer ran freely 10 feet down the 25° plane before the slack was all taken up kinetic energy of nearly 6 foot-tons would be created, while the stress of impact would be about 14 tons per square inch at the start,

FIG. 2.

but doubled at each reflection of the wave in the short link. This wave of stress itself is of decreasing intensity corresponding to the reducing velocity of the trailer as it is being retarded.

Incidentally, the author may state that he has met with several cases of "overwind" in which the rope has not been broken, but, has shown by numbers of "waists" on the wires of the most stressed parts that ductility has been the saving grace. This quite disposes of the idea, that has sometimes been advanced, that in the case of this sort of impact at fairly high speed there was not time for ductility to assert itself. Where the wires of a rope break due to shock without local elongation. these wires had lost their ductility and were brittle.

Coming now to the photographs of the fractures obtained in the test specimens the fibrous nature of the annealed one (Fig. 3) will be noticed contrast to the crystalline fracture in the other (Fig. 4). The reduction in area in the former case is very marked. The material of which the ring is made appears to be of at least two different qualities of iron. Under the microscope this shows up very plainly in the unnannealed specimens (Fig. 4), the crystals in the various bands being of widely different sizes. Microscopically viewed, similar differences

TABLE

RESULTS OF TENSILE TESTS ON SAMPLES TAKEN FROM TRAILER RING.

No. of Test.	Length	Diameter.	Area in Sq. Ins.		Percentage of reduction at fracture.	Percentage of Elongation.		Elastic Limit.		Maximum.		Remarks.
			Before Test.	After Test.		In 4in.	In 2in.	Load. Lbs.	Stress per Sq. In. Lbs.	Load. Lbs.	Stress per Sq. In. Lbs.	
	in.	in.										
A	9·5	1·3125	1·353	1·320	2·4	10·93	12·50	57,500	42,498	80,340	59,379	Broke at one end clear of grips. Fracture—coarse crystalline.
B	10·75	1·375	1·4849	1·1075	25·4	In 8in. 17·20	28·12	53,960	36,339	86,600	58,320	Sample was throughly annealed. Broke at one end just clear of grips. Fracture fibrous

occur in the annealed specimen. However, as previously stated, the object of annealing was achieved, viz., the securing by *simple heat treatment* of an access of ductility and extensibility.

The mining regulations of different countries generally prescribe for the regular

FIG. 3.

annealing of chains and connections used in the winding of persons. The practice should extend to all chains and connections used in the shaft, care being taken not to overheat these fittings and to allow for very gradual cooling in the case of those made of iron.

Reference to the following British reports dealing with the subject will show that regular annealing is strongly recommended:

Report of Professional Committee appointed by the Royal Commission on Mines, 1909, pages 28 and 59.

FIG. 4.

Report on the Construction, etc., of Hoists and Teagles, 1904, by William S. Smith, Esq., H.M. Inspector of Factories, pages 10 and 27.

THE PRESIDENT, in moving a vote of thanks to the author, said Mr. Vaughan

had given them an interesting example of what a short paper should be. Short papers of that type really formed as valuable a contribution to their proceedings as many a lengthy paper. He hoped they would have more short papers of a similar nature.

E. J. LASCHINGER (Past-President) expressed the view that a recommendation should have been made by Mr. Vaughan in his paper as to how the annealing should be done. Annealing was only a term describing many different processes to attain a certain end, and it was quite possible to imagine that what some might call annealing was quite an ineffective process. He suggested that Mr. Vaughan might supplement his paper by giving a practical instruction as to what kind of annealing would be effective.

THE PRESIDENT (Mr. Bernard Price) said that if they could get the members who were responsible for that kind of work to mention the ordinary methods in vogue it would lead others to suggest means.

W. R. LAURIE (Member) referred to his experience with chains which had been taken off a dredger. Some of them had crystallised and broken, but on being heated in a blacksmith's fire to a cherry red and left to cool in ashes, no further trouble had been experienced with the chains. This process was performed every six months, and was in vogue thirty years ago.

H. W. CLAYDEN (Member) thought that the usual method of annealing carried out on the mines was to heat to a cherry red and to allow to cool in ashes.

E. J. LASCHINGER (Past President) saw no reason why a simple kind of furnace could not be designed to do the annealing in an effective manner. The expense should not be a large matter. Duplicate connections for skips were always available on the mine, and if one went wrong there was usually a spare ready. When a connection, therefore, had been in use for a certain period a spare set could be put in and the former could be annealed. Considering that one of the safety measures was that skip and cage connections should be of the very best kind, the matter was of great importance, and he thought the question ought to be gone into very thoroughly.

A. C. WHITTOME (Past President) said that Mr. Laschinger had asked Mr. Vaughan to describe what was a very well-known process to every engineer on these fields, and to give a better process than the usual one. Rather than wait for some highly refined method to be laid down by which annealing should take place, it was better to use any system. Engineers generally were under the impression that only portions of the connections between skips and ropes were to be annealed, but under the Regulations all portions of these skip connections, for skips in which men were conveyed, had to be annealed. Annealing was simply bringing the metal back to its original structure, and any method which would accomplish this would be proper annealing of that metal.

W. ALDERSON (Member) referred to the restoration of the molecular structure of steel by a rest cure, and said that he remembered some tests which had been carried out in the early days on locomotive tyres. The tyres were hung up unheated in the shops, and a hammer was arranged to beat on them at regular intervals over long periods. After a certain time the tyres usually fractured. Pieces from the tyres so tested showed a highly crystalline structure when broken, but after being laid aside for a lengthy period and retested for tensile strength, etc., they gave results equal to the steel before the hammering tests, while the crystalline appearance had disappeared, and the steel in every way appeared as in the original forged tyre.

Unless annealing of steel was carried out in a proper furnace, where all air was excluded as far as possible, oxidization took place. From experiments he had noted that repeated annealing shortened the time to resist fatigue. The granular structure of the metal seemed in time to get coarser, so that he thought that annealing could be done too often. He had not, however, seen any tests carried out as regards tensile strength of steel after repeated annealing and fatigue. A well-known illustration of the value of rest was shown in the ordinary razor blades; these, as every man knew,

recovered their " keenness " if allowed to rest after being in use for some time.

J. Whitehouse (Member) said that lest the impression should be gathered from Mr. Whittome's remarks that annealing on the mines was generally carried out in a slipshod manner, he would like to point out that the Village Deep Company had a special annealing furnace, in which all the work was enclosed during the process of annealing, and in that case the fire was allowed to die away and the material cooled down gradually. By enclosing the work in a receptacle of this kind it was protected from the action of injurious gases.

Another point which Mr. Vaughan had raised, which, however, had nothing to do with annealing, was that the trailer referred to in his paper was connected by ten feet of wire rope. It occurred to the speaker that if that connection had been shorter the chance of the link breaking would have been reduced, since the trailer would have had little opportunity to get up a high velocity before its weight was again taken by the connection to the skip.

A. C. Whittome (Past President) said he had not meant to convey that slipshod methods were in use on the mines, but that a good many engineers did not realise that the whole of the connections of a skip had to be annealed. He did not question the fact that annealing on the mines was done in as perfect a manner as it was possible to do it, but the point he objected to was that Mr. Laschinger wished Mr. Vaughan to lay down some ideal method of annealing. He thought that if the connections between rope and skip were simply heated and cooled in air this would be better than no method of annealing.

J. A. Vaughan (Past President) thought he had made it clear that those portions of the skip connections which were legally liable to be annealed, were usually annealed. He was alluding in his notes to portions of the connections used purely for rock work. He presumed that the Regulations as laid down were being properly dealt with. He knew of one mine where duplicate bridles to the skips were provided, and these were changed regularly every six months. In the particular instance he referred to in his paper the point raised by Mr. Whitehouse had been a source of weakness. The length of rope allowed the trailer a free fall of 10 feet down the incline of 25°. The connecting link if 10 feet, however, would have been of service and not a source of weakness in any case of shock on the upward journey.

E. Farrar (Past President) did not agree with the suggestion made by Mr. Whittome of cooling in air.

G. A. Denny (Past President) said he agreed with Mr. Laschinger that Mr. Vaughan's paper seemed to point to the conclusion that there was an immense safety factor to be obtained by annealing. Mr. Laschinger appeared to him to take a logical attitude in maintaining that if they were to anneal they should have a system which would be regular and acknowledged in all parts and known to engineers as the most effective and practical one. He supported the view that Mr. Vaughan should be asked to look into the matter still further and recommend a method which would be at once practicable and efficient.

Form the " Journal of the South African Institution of Engineers." Vol. XVI, No. 4, 1917.

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

By. J. A. Vaughan (Past President).

It is the universal practice to regard the factor of safety of a wire rope used for winding in mine shafts as the quotient resulting from dividing the breaking strength of the rope by the sum of the maximum load to be hoisted *plus* the total weight of the rope in the shaft when the conveyance is at the lowest station.

In most countries the breaking strength of the rope is taken as that given in the manufacturer's certificate. Some manufacturers assess the strength of the rope as the sum of the strengths of the composing wires, while others make some deduction.

In the Union of South Africa the breaking load under actual test is taken as the strength of the rope. Each coil, if not tested in the country of manufacture, has to be sampled in the Mines Department Mechanical Laboratory.

The differences existing between the estimated and the actual test breaking load for any of the usual sizes or constructions of rope are fairly well known, and will not be further referred to at this stage.

It may be stated here, however, that in all calculations presented in this paper, the strength of the wire rope will be taken as the product of 108 short tons into the sum of the sectional areas of the wires forming the rope. The value of 108 tons (of 2,000 lbs.) is taken as being 90 per cent. of 120 tons per square inch, representing a class of wire very commonly used on the Rand.

The writer at this point does not propose to argue whether this class of wire is the best class to use. He would point out that the adjustment of the formulae and calculations submitted later to take account of any difference in breaking stress of steel or deduction for " laid-up " efficiency of the wire is a simple matter.

In recent years various commissions and committees have presented reports dealing with mining matters and including the particular point the writer has now under consideration.

About 1901 the Dortmund (Germany) Commission reported that experience had shown that the minimum factor of safety prescribed by the Mining Regulations, viz., 6, was sufficient.

In 1907 the Transvaal Commission reported that a minimum factor of safety of 6 was sufficient, dealing with the winding ropes at work and not when about to be put on the drum. Although they felt that, in deep level winding, the long rope should receive some consideration on account of its better ability to meet the shocks incidental to winding, they were not prepared to disregard other stresses, such as those due to bending, to which the rope was subjected. These stresses had not been exactly determined or proved to be independent of of the depth of winding. The writer agreed with this finding at that date, viz., 1907, but since then, from fatigue experiments on steel wire, conducted in the Mines Department Laboratory, he has come to the conclusion that the bending stress is of little importance when, as in good practice, the ratio of D:d is kept as high as 1,000.

In 1909—the Royal Commission on Mines, Great Britain, agreed with the report of its professional committee that 3½ years should be the maximum life of a winding rope. The committee found that the factors in vogue varied from 6 to 10, but noted that even the minimum factor of safety would be difficult to maintain in cases of great depth. They favoured in preference the maintenance of a " reserve of strength," but did not specify what this reserve was to be or how it was to be attained.

In 1915, in Bulletin 75 of the Bureau of Mines, Washington, U.S.A., there was published the draft for a mining law, with explanatory notes, prepared by a committee appointed by the American Mining Congress in 1906. In section 34 of this draft law the following provisions occur:—

" The factor of safety of all such ropes or cables when newly installed in shafts less than 3,000 feet deep shall in no case be less than six, and shall be calculated by dividing the breaking strength of the rope, as given in the manufacturers' published tables, by the sum of the maximum load to be hoisted, *plus* the total weight of the rope in the shaft when fully let out.

" It shall be unlawful to use any rope or cable for raising or lowering of men when its factor of safety, based on its existing strength and dead load, shall have fallen below 4·5.

" It shall be unlawful to use any rope or cable of the so-called 6 by 19 standard construction for the raising or lowering of men, either when the number of broken wires in one lay of said rope exceeds six, or when the wires on the crown of the strands are worn down to less than 65 per cent. of their original diameter, or when the superficial inspection provided for in this section shows marked signs of corrosion: Provided, however, that when such broken wires are reduced by wear more than 30 per cent. in cross section, the number of breaks in any lay of the rope shall not exceed three."

The writer would have taken the minimum factor of 4·5 to apply to all shafts, were it not that on page 112 the Committee state: " The code therefore imposes a minimum safety factor for all installations and another below which no rope shall fall and remain in service; very deep shafts it exempts altogether."

In discussing the figures 6 and 4·5 for original and final factors of safety the Committee advised mining operators in their own interests to conform to the specifications stated in Chapter V. of the Report. These specifications were drawn up by an advisory committee formed from the leading rope manufacturers in the country. These recommendations are shown in the following table:—

Hoisting-rope safety factors for various depths of shafts.

Length of Rope. Feet.	Minimum safety factor for new rope.	Minimum safety factor when rope must be discarded.	Percentage Reduction.
500 or less ...	8	6·4	20
500 to 1,000 ...	7	5·8	17
1,000 to 2,000...	6	5·0	16⅔
2,000 to 3,000...	5	4·3	14
3,000 and over	4	3·6	10

The framers of this table explain that the basic principle is an actual safety factor of four. This figure is increased for the shallower shafts because in them the stresses due to acceleration and extraordinary circumstances are normally greater, there not being sufficient spring in the rope to moderate the effect of shocks.

This proposal may be designated the "stepped" factor as contrasted with the fixed factor of safety.

Other propositions have been advanced by various individual engineers, the most interesting being the composite formula "$m\ a + n\ b$," "m" *and* "n" being numerals while "a" and "b" represent the weight of load and rope respectively. This formula of safety gives the breaking load of the winding rope.

In the discussion on Mr. R. B. Greer's paper in 1912, Mr. J. F. Cook proposed the formula "$8a+b$" as giving a constant "margin of strength."* Mr. Greer advocated the modification of the present six factor of safety by a sliding scale after a depth of 2,500 or 3,000 feet.

The writer in 1904† was of the opinion that for all depths greater than the dangerous limit the maintenance of a standing reserve of strength in the rope appeared a suitable provision against the effects of kinetic shock, and that this reserve might be secured by the use of a sectionally tapered rope.

The dangerous limit alluded to was what might be called the "critical depth" for each rope of a certain lower-end-loading, within which depth the effect of a shock of given magnitude was augmented, owing to reflections of the wave of stress, and became much more intense than at this or a greater depth. The calculations dealing with this and some allied matters are dealt with in an appendix to this paper.

Diagrams have been prepared showing the effect on the capacity of the rope of the adoption of each of the safety systems previously mentioned.

Investigation shows that no one of these systems is quite satisfactory, and the writer now proceds to introduce an alternative scheme which appears to him to be sound and to possess at least the quality of simplicity.

The "critical depth" below which the kinetic shock for a definite sudden change of velocity is either constant or diminished, is, as shown in the appendix, at a point where the length of the rope weighs very nearly ·35 (actually $\frac{1}{3}$ log 2 = ·3466) of the load carried at its end.

The following steps are taken with the idea of utilising this result as a justification for the reduction (below six) of the factor of safety of winding ropes in the deeper shafts:—

Let "a" denote the supported load (tons) at end of rope.
Let "b" denote the weight of rope (tons).
Let "y_1" denote the factor of safety at lower end of rope.
Let "y_2" denote the factor of safety at upper end of rope.
Let "s" denote the cross wire section of rope (sq. inches).
Let "x" denote any specified vertical depth (feet).

It is a fact that new rope, of usual construction, of 1 sq. in. wire section weights, very approximately, 4 lbs. per ft., and that ropes of larger or smaller section weigh proportionately.

* "The Journal of the South African Institution of Engineers," Vol. XI., No. 4, page 126.
† "The Journal of the South African Association of Engineers," March, 1904.

The steel that will be considered initially is of 120 tons per sq. in. reduced in efficiency to 108 ton stuff as laid up in the rope.

It will be seen that the following relations exist:—

$$a = \frac{180}{y_1}s, \quad b = \frac{4x}{2{,}000}s.$$

$$\text{Now } y_2 = \frac{\text{Strength of rope}}{\text{Maximum load}}$$

$$\text{So that } y_2 = \frac{108s}{\left(\frac{108}{y_1} + \frac{4x}{2{,}000}\right)s} = \frac{108}{\frac{108}{y_1} + \frac{x}{500}}$$

$$\text{whence } x = \frac{(y_1 - y_2)\ 54{,}000}{y_1\ y_2} \qquad (1)$$

If $y_2 = 6$, or $y_2 = 5$, or $y_2 = 4{\cdot}5$ then this equation becomes:—

$$x = \frac{y_1 - 6}{y_1}\ 9{,}000 \qquad \text{(IIa)}$$

$$x = \frac{y_1 - 5}{y_1}\ 10{,}800 \qquad \text{(IIb)}$$

$$x = \frac{y_1 - 4{\cdot}5}{y_1}\ 12{,}000 \qquad \text{(IIc)}$$

In the equation $x = \frac{y_1 - 4{\cdot}5}{y_1}\ 12{,}000$, the value of x for various values of y_1 are as follows:

y_1 =	6,	7,	8,	9,	10.
x =	3,000	4,286	5,250	6,000	6,600.

To obtain the critical depth for various values of y_1 it is necessary to substitute these values in the equation:—

$$\frac{4x\ s}{2{,}000} = \frac{35}{100}a = \frac{35}{100} \times \frac{108s}{y_1}$$

$$\text{whence } x = \frac{18{,}900}{y_1}. \qquad \text{(IIIa)}$$

or from substituting in Equation (I):—

$$x = \frac{14{,}000}{y_2}. \qquad \text{(IIIb)}$$

so that if $y_1 = 6$, 7, 8, 9, and 10 successively x will = 3,150, 2,700, *2,362*, 2,100, and 1,890. When these depths are compared with the depths obtained from equation IIc., shown earlier, and also with those obtained from equation IIa. (in which $y_2 = 6$ as at present on the Rand), viz., 0, 1,285, *2,250*, 3,000, and 3,600 it will be seen that, in the case of $y_1 = 8$, the values nearly coin-

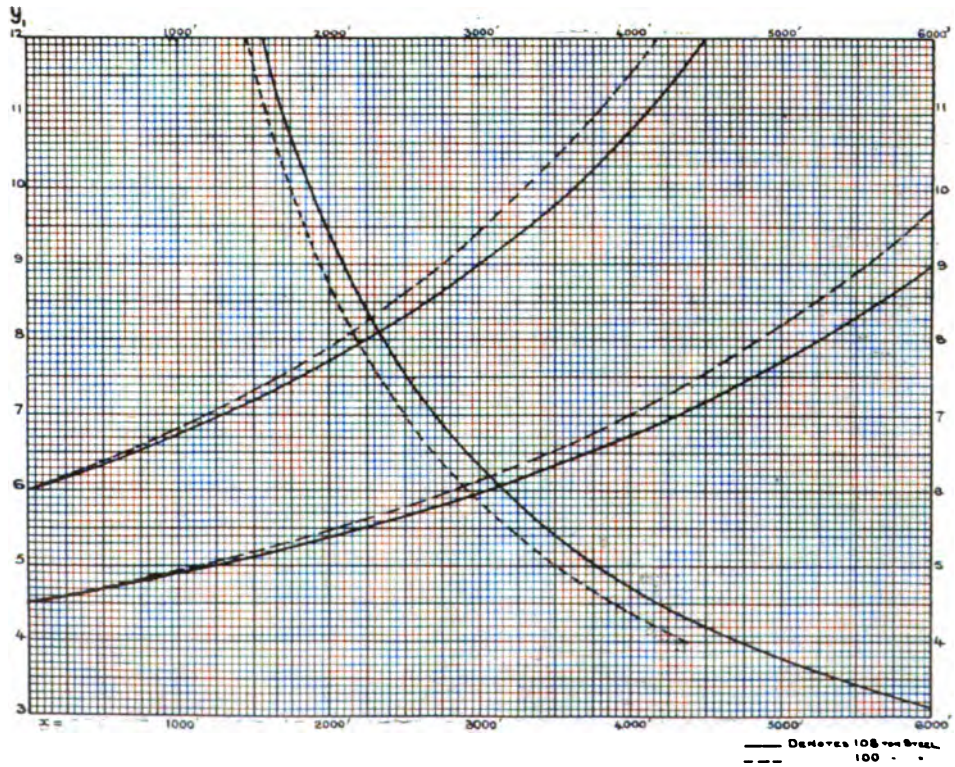


DIAGRAM 1.

cide, and, that for higher values of y_1, the increases of winding depth that would be obtained by the adoption of 4·5 as a factor of safety are all outside the limiting position for augmentation of kinetic stress arising from wave reflections. Diagram I. illustrates this point and shows the curves derived from equations IIa., IIc., and IIIa.

The curves in dotted line show the effect of a reduction in the breaking load of the steel from 120 tons to 111 tons per square inch.

It can be calculated, and may be observed on the Diagram I, that the surves IIa. and IIIa. meet at the point $y_1 = 8{\cdot}1$, $x = 2{,}333$.

If B denotes the available breaking stress of the steel wire in the rope, *i.e.*, say 90 per cent. of the actual, then Equation I. becomes:

$$x = \frac{y_1 - y_2}{y_1 y_2} 500B$$

and Equation III becomes:

$$x = \frac{175}{y_1} B$$

These curves, for the value of $y_2 = 6$, meet at the point $x = 21{\cdot}6B$; $y_1 = 8{\cdot}1$, this latter value being independent of B.

Diagram 2 shows, with y_2 as ordinate, the curves denoted by Equation (1) after the substitution of various values of y_1; and also the curve of "critical" depth. It will be seen that the "critical" curve meets the curve $y_1 = 8$ at a depth of 2,362 ft., while this latter curve cuts the horizontal line $y_2 = 6$ at 2,250 feet.

Considering that theoretical results are about to be applied to a practical problem and that there is every probability of the *speed* as well as the *strength* of the wave of kinetic stress being reduced in the practical case, the writer feels perfectly justified in assuming that the "critical" curve falls within the 2,250 feet mark on the $y_2 = 6$ line, that is to say, to the left of the point where the curve $y_1 = 8$ cuts this line.

The factor y_1, which may be called the "capacity-factor" of the rope, denotes how many times its breaking load is greater than the load it has to transport.

In winding a load, of, say, 10 tons, from a depth of 5,250 feet to the surface, with a

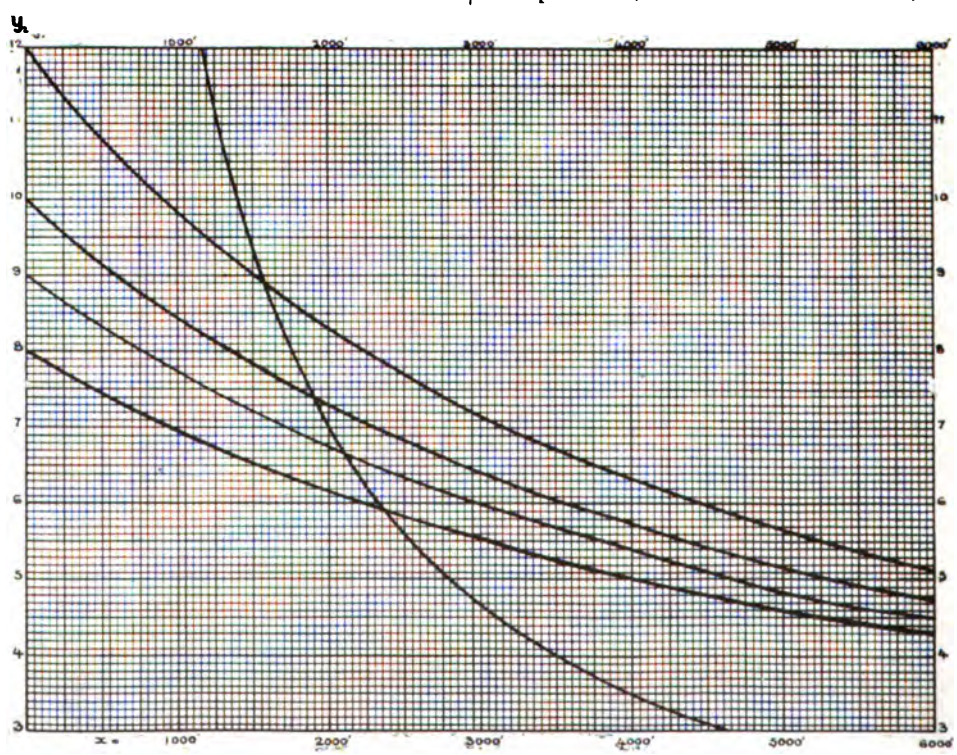


Diagram 2.

rope of 80 tons breaking load, the curve, $y_1=8$ shows what the factor of safety (y_2) will be when the conveyance is at any particular depth, *e.g.*, 5·02 at 4,000, 5·538 at 3,000, 6·17 at 2,000, 6·97 at 1,000, and 8 at the surface where there is only a small weight of rope to effect any difference between y_2 and y_1.

Seeing that a minimum factor of safety 6 has been found sufficient at shallow and moderate depths, and seeing that the rope denoted by $y_1=8$ provides for a factor of safety of 6 within the danger zone, *i.e.*, the area to the left of the critical curve in the Diagram 2, the writer proposes to allow this rope, or a rope with the same capacity-factor, to go down further, to depths where the factor of safety is 5 or even 4·5.

If a larger rope is used in order to attain a factor of safety of more nearly 6 outside the danger zone, then this rope will have an unecessarily high factor of safety within this zone. It would be represented on Diagram 2 by the curve $y_1=8·5$ or $y_1=9$.

A rope of capacity-factor of 8 may not have actually its greatest reserve of strength at a point just beyond the critical curve for the following reasons:—

(1) The intensity of the wave of stress due to shock in all probability diminishes during its passage along the rope.

(2) The longest rope possible is at the greatest advantage in the case of certain shocks.

From long experience in winding operations it has been found that a rope with a minimum capacity-factor of $y_1=8$ is strong enough to withstand stresses due to both static and kinetic conditions up to a certain depth where $y_2=6$. In the midst of uncertainty and conjecture it is, however, known that the kinetic stresses are less below this depth, in some cases the difference increasing with depth. Is it not, therefore, sound practice to allow the static stress to slightly increase?

Some reduction of y_2 may be adjudged to be permissible, and this reduction should be gradual in proportion to the depth. A constant capacity-factor such as $y_1=8$ meets the case, but the question remains to be answered as to what limit should be set to the reduction in y_2.

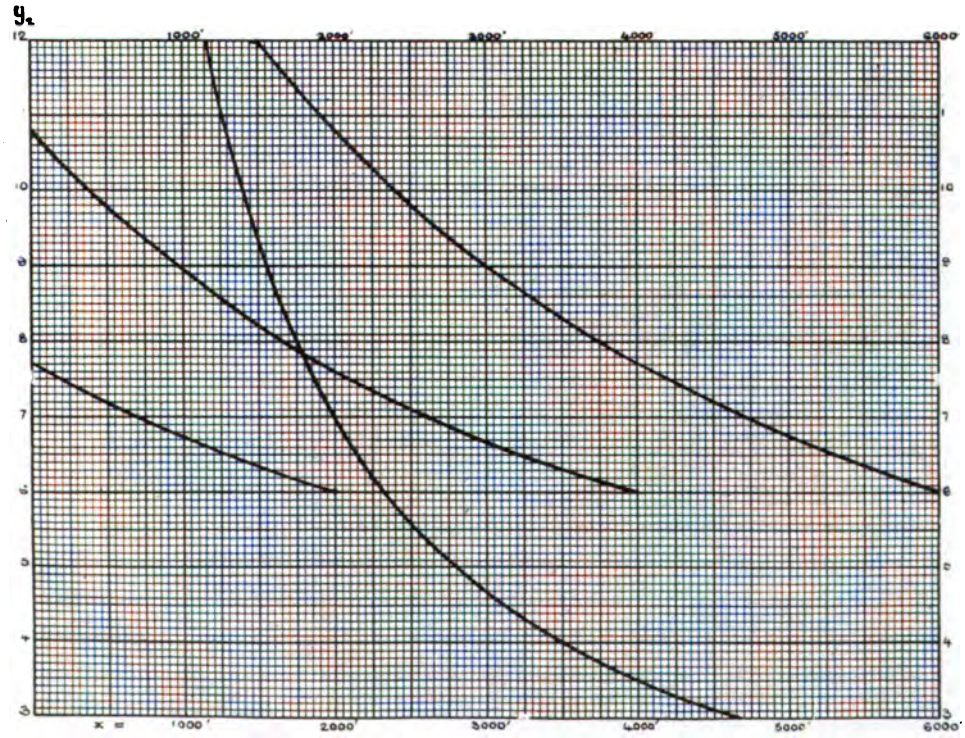


Diagram 3.

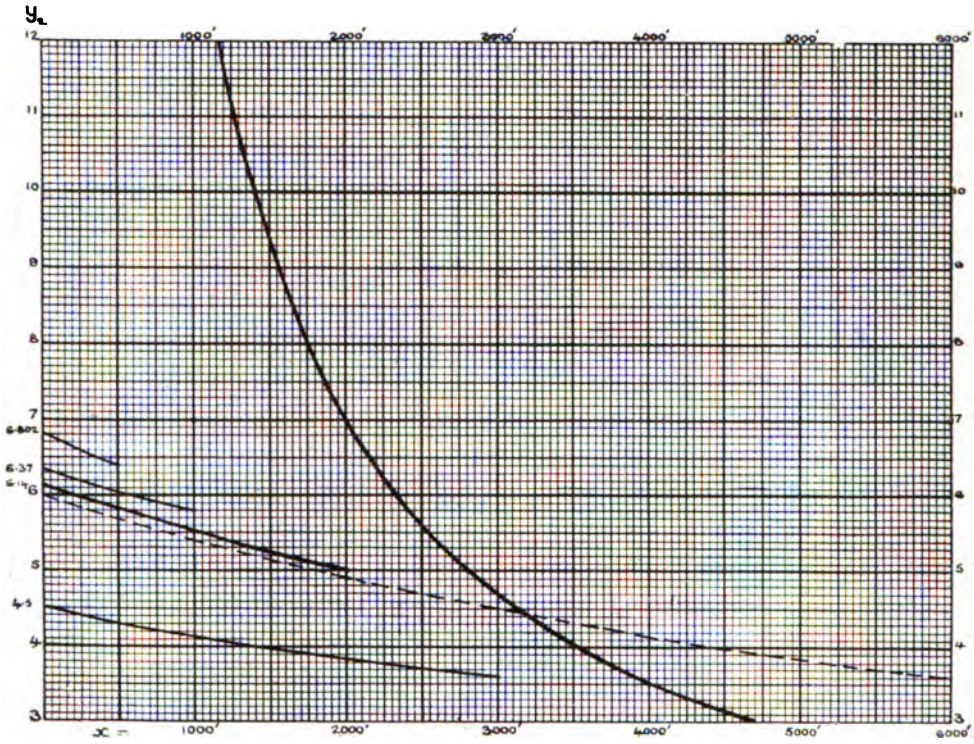


DIAGRAM 4.

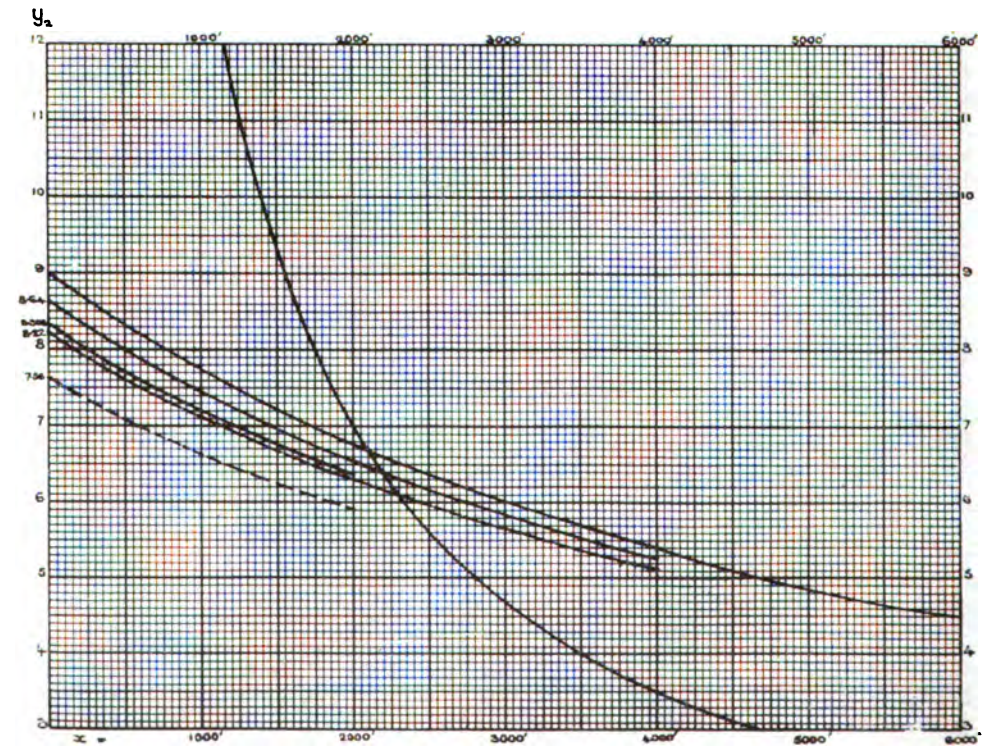


DIAGRAM 5.

Before answering this question it is best to carefully consider the other proposals that have been made in the way of safety factors of formulae.

Diagram 3 illustrates the old conservative idea of the fixed factor of safety. This system is extremely harsh on all deep level mining schemes unless tapered ropes are used. Taking for minimum value $y_2 = 6$, for 2,000 feet the capacity factor will be 7·7, for 4,000 feet 10·8, and for 6,000 feet the enormous value of 18. These values of y_1 also give directly the values of y_2 when the load is at the surface, whence the differences in the reserves of strength that have to be maintained in the ropes may be readily seen.

Diagram 4 illustrates the effects of the application of the "stepped"-factor of safety system, as advised by the U.S.A. manufacturers. Its inventors appear to have overlooked the fact that, in winding from 2,000 feet, the 1,000 foot step has to be passed, and that, in winding from 4,000 feet, all the intermediate stages have to be negotiated. This system calls for a minimum factor of safety of 5·8 at 1,000 feet, but allows a factor of 5 directly the 1,000 feet depth is passed, and so on at each step. Finally, it permits a minimum factor of safety of 3·6 for 3,000 feet and over. The winding rope for 6,000 feet would have a capacity-factor of 6, while that for 3,000 feet would have only 4·5.

Diagram 5 shows for depths of 2,000 feet, 4,000 feet and 6,000 feet the effect of using either of the composite formulae $8a + b$ or $7a + 2b$ in deciding the breaking load of the winding rope to be adopted.

Both these formulae result in the same rope of capacity $y_1 = 9$ for 6,000 feet, where $y_2 = 4·5$.

The $8a + b$ formula becomes $y_1 = 8·64$ at 4,000, where $y_2 = 5·26$; and $y_1 = 8·308$ at 2,000 feet depth, where $y_2 = 6·353$.

The $7a + 2b$ formula becomes $y_1 = 8·22$ at 4,000, where $y_2 = 5·102$; and $y_1 = 7·56$ at 2,000 feet, where $y_2 = 5·906$.

The latter results in values of y_2 below 6 within the critical depth, while the former (*i.e.*, $8a + b$) errs on the generous side within this danger zone, being always higher than $y_1 = 8$.

Tapered ropes, generally sectionally tapered ropes, have been used in a few cases in deep levels on the Rand to maintain the legal minimum factor without accumulating an undue reserve of strength. The best friends of this class of rope will hardly claim that it has been a great success. They have to be especially designed for each shaft and are naturally expensive.

By reducing the factor of safety below 6 after the critical depth is reached the necessity for tapered ropes is done away with.

If a winding rope is installed of capacity factor $y_1 = 12$ it can be allowed to wear down one-third before it reduces in capacity to $y_1 = 8$. The writer proposes this practice for general adoption considering that it would not create any hardship in the case of any shaft under 2,000 feet in depth.

The limit of $y_2 = 4·5$ appears a reasonable one to adopt in the light of American experience in deep level winding. Between the depths of 5,250 feet and 6,000 feet the 4·5 limit could be maintained by allowing less than one-third loss of strength in the rope, or, as shown later, by initially adopting a higher value for B.

The Regulation dealing with the relation of breaking load of the rope to the maximum working load would require to be entirely altered, and should read somewhat as follows:—

No winding rope shall be used when the breaking load at any point therein has become reduced to less than eight times the load carried at its lower end, provided, however, that in no case shall its factor of safety, based on its existing strength and maximum working load be permitted to fall below 4·5.

The latter provision in the proposed Regulation is necessary if shafts of over 5,250 feet in depth are taken into consideration, if steel of unnecessarily low ultimate stress is used in the manufacture of the rope, or in case of abnormal wear in the upper part of the winding rope.

The initial value of y_1 as found in the vertical shafts on the Rand are as shown in the following table, from which it will be seen that the proposal to start off at $y_1 = 12$ and permit wear down to $y_1 = 8$ will afford relief to the deeper levels without pressing hardly on the shallower mines.

Depth of Shaft, in feet.	Average value of Capacity. factor = y_1.	
	Calculated for maximum load of person plus weight of conveyance.	When hoisting ore.
Less than 1,000 ...	18·33	14·02
1,000 to 2,000 ...	17·0	11·08
2,000 to 3,000 ...	20·08	14·52
3,000 to 4,000 ...	21·73	14·17
4,000 and over ...	22·4	15·3

In sorting out the values of y_1 shown in the preceding table an opportunity was afforded of checking the rule on which the writer's formulae were based, viz., that rope of 1 square inch wire section weighs 4 lbs. per foot. In the licensed hoists on the Rand there are 101 ropes working in vertical shafts. These ropes range in size from 0·78 inch diam. to 2 inch diam., or from 1 lb. to 7 lbs. per foot. The ultimate stress of the steel varies from 100 to 149 tons (of 2,000 lbs.) per square inch.

The breaking load of each rope was calculated and then compared with the actual test result. The calculation consisted in dividing by 4 the product of weight per ft. $\times 90$ per cent. of the ultimate stress of steel. The actual breaking load was slightly greater than the calculated B.L. in 40 instances and slightly less in 61 instances. The average actual B.L. of the 101 ropes was 74·48 tons, while the B.L. calculated from the average ultimate stress and average weight per ft. was 75·67 tons.

In the 40 instances just mentioned the average weight of the rope per ft. was 1·1973 lbs., while in the 61 instances the average was 3·058. Ropes of all intermediate sizes and of various types of construction were included in each category, and no definite modification of the rule to suit either size of rope or type of construction can be now suggested by the writer, but it is evident from the average results that the heavier ropes are somewhat less efficient than the lighter ones if the initial test breaking load is accepted as a standard.

It requires to be remembered what is really meant by the curve designated, for the sake of brevity $y_1 = 8$. It is the curve, for various values of x and y_2, obtained by substituting this particular value of y_1 in the equation.

$$x = \frac{y_1 - y_2}{y_1 y_2} 500 \text{ B.}$$

Substituting this value of y_1 the equation becomes

$$8xy_2 = (8 - y_2) \ 500 \text{ B.}$$

If $y_2 = 4·5$ and B = 90 per cent. of 130 = 117, then x = 5,688 ft., while if $y_2 = 4·5$ and B = 90 per cent. of 140 = 126, then x = 6,125 ft. The number 500 was arrived at by dividing 2,000—the number of pounds in a short ton—by 4, the alleged weight per foot of a rope of one square inch wire section.

It is evident, therefore, seeing the position of the quantity 500 B in the equation, that corrections to suit all circumstances, including loss of efficiency in the rope, *i.e.*, the lowering of its strength in comparison with its weight, can be introduced readily by an adjustment of the value of the numerical factor.

The simplicity of the writer's propcsal has been alluded to earlier in the paper. This point need hardly be enlarged upon, but it may be just mentioned that, in selecting a rope for a winding proposition, the fact that the depth of the shaft may be left out of the calculation is of some little assistance.

This investigation has only been concerned with vertical shafts, but its application may, with modifications, be extended to incline and compound shafts.

Appendix.

In this appendix are given the calculations for estimating the stress occasioned in the winding rope (" x " feet long, of weight " b " tons and carrying a load of " a " tons) by a kinetic shock at velocity " v " feet per second occurring under three different circumstances, described under Cases I., II. and III., the wave of stress travelling at the rate of V feet per second.

The rope is regarded as a single wire of section " s " square inches.

In translating the results the following points have to be remembered:—

(1) A wave transmitted along a wire rope will probably suffer considerable diminution in intensity due to the friction between the strands and wires as it travels, and, this being so, the stress will not reach such high values as in the case of a single wire.

(2) The reflections at the top and bottom ends of the rope will be by no means perfect, especially so in the case of the former.

(3) The length of the wire spiral may be from 5 to 10 per cent. greater than the length of the rope, so that, if the wave splits up into components along the separate wires the rate of travel will be lessened.

By neglecting to take account of these considerations and applying the theoretical results directly to the case of a wire rope the error is on the side of safety.

It would be extremely interesting to evaluate these considerations by means of practical tests in a deep mine shaft.

Case 1. The rope is lowering the load "a" when motion of the former is suddenly stopped.

A wave of stress, $S = E\frac{v}{V}$, travels down the rope and is reflected when it reaches the lower end, stress 2S resulting. The force retarding the motion of the load (equal to "a" tons) consists of two parts, one of which is constant, viz., $P = K.E.\frac{v}{V}$, and the other part, viz., $K.E.\frac{v_r}{V}$ varies with the velocity of the load at the moment.

Equation of motion:—

$$\frac{a}{g}\frac{dv}{dt} = K\frac{E}{V}(v + v_r)$$

$$\text{whence } dt = \frac{a}{g}\frac{V}{E.K}\frac{dv}{v+v_r}$$

Integrating between limits $v_r = o$ and v we

$$\text{obtain } t = \frac{a}{g}\frac{V}{EK}(\log_e 2v - \log_e v)$$

$$\text{whence } t = \frac{a}{g}\frac{V}{EK}\log_e 2$$

If L feet be the length of the wave $L = 693\frac{a}{g}\frac{V^2}{EK}$, independent of "v"—the shock velocity.

Substituting the value $V^2 = \frac{E}{\rho}$

$$L = .693\frac{a}{g\rho K}$$

or $L\,g\rho K = .693\,a$.

Thus the weight of rope included in the wave is equal to about ·7 of the weight of the load carried. If the length of rope, x, is equal to one half L, the wave will, after reflection at the upper end (where the stress will be increased to 3S), return to the lower end just as the load "a" has been brought to rest.

If the rope is shorter than $\frac{L}{2}$, or less than ·35 a in weight then the stresses mentioned will be increased, as illustrated in Diagram 6.

When "x" is such that $b = \frac{35}{100}a$, this depth may be called the "critical," the shock being greater for lesser depths, but no less for any greater depth.

Case II. A load "a" with velocity v downwards is suddenly attached to the end of the rope previously at rest.

This may occur as the result of the cage sticking in the shaft while being lowered and slack rope accumulating. The driver perhaps notices something wrong and stops winding. Before the slack rope is taken up, the cage may free itself and fall.

The lowest section (infinitely thin) of the rope is instantly jerked into motion downwards with velocity v feet per second. This section communicates its motion to the section next above, and would thereby come to rest were it not that it is still moved downward by a, but now, with reduced velocity, some of the kinetic energy of a being expended.

A wave of stress travels up the rope, of head $E\frac{v}{V}$ and of gradually reducing intensity.

The load is experiencing retardation proportional to its velocity, and cannot mathe-

DIAGRAM 6

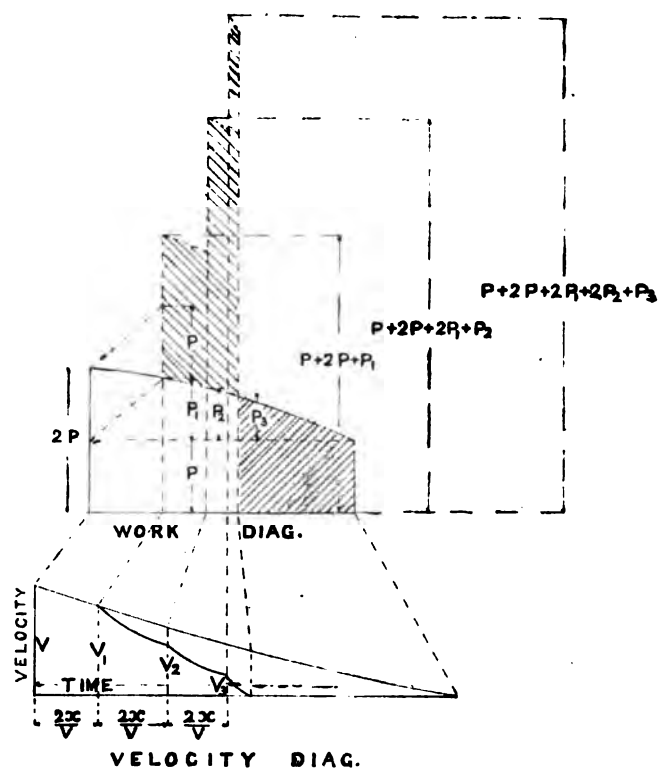


Diagram 6.

matically speaking, be stopped until the wave reflected from the top end has reached it.

Equation of motion of " a " downwards:

$$\frac{a}{g}\frac{dv}{dt} = K.\frac{E}{V}.v_r$$

Integrating between limits $v_r = v_1$ and v

$$t = \frac{a}{g}\frac{V}{EK}(\log_e v - \log_e v_1)$$

Substituting for "t" the time $\frac{2x}{V}$ taken by the wave to go up and down the rope,

we find :—$\log_e \frac{v}{v_1} = \frac{2 x g EK}{a V^2}$

$$= 2\frac{x g \rho K}{a}$$

$$= 2\frac{b}{a}$$

If $b = .35\ a$, then $\frac{v}{v_1} = 2$ and the velocity has been reduced to one half.

The wave now doubles on itself and goes up the rope again, the force retarding a's downward motion at that instant being $2\frac{1}{2}$ times $K\frac{E.v}{V}$.

This rope is evidently long enough to ensure that the motion of a downward may be arrested before the next return of the wave. If the rope were not long enough a still greater stress would be created at the subsequent returns of the wave.

The longer the rope the more nearly will the maximum stress at the lower end reduce to 2 S where $S = E\frac{v}{V}$.

The least length of rope could be calculated which would allow of the downward motion of the load "a" being arrested just before the head of the wave reaches the lower end of the rope for the second time.

For the purpose of this paper, however, it is sufficient to have shown that the depth calculated for Case I. provides safety in this Case II.

Case III. Starting to hoist with the rope slack, so that the load is suddenly jerked into motion.

If the rope is suddenly given a velocity v feet per second, the lower end when the wave of stress reaches it would, if there was no load attached to it, respond with velocity 2 v. The load being there, no motion of the lower end of the rope can take place until this load is accelerated upwards. The equation of motion will be:—

$$\frac{a}{g}\frac{dv}{dt} = 2k\frac{E}{V}v - k\frac{E}{V}v_r - a$$

Integrating between the limits $v_r = o$ and v we find $t = \frac{a}{g}\frac{V}{Ek}\log_e\frac{2Cv-a}{Cv-a}$ where $C = k\frac{E}{V}$

If $t = \frac{2x}{V}$

$$\log_e\frac{2Cv-a}{Cv-a} = \frac{2b}{a}$$

If $Cv = 1{\cdot}1a$	then $b = 1{\cdot}242a$
$Cv = 3a$	$b = {\cdot}458a$
$Cv = 100a$	$b = {\cdot}35$ approximately.

The advantage of a long rope is apparent, as it will allow the load to be fully accelerated before the return of the wave of stress to the lower end of the rope.

The shock velocity here appears as the determining factor in the length of the wave. Although for moderate shock velocities the "critical" depth arrived at in Case I. is exceeded, it has to be noted that the extreme shock is only likely to occur at starting the upward trip, when the rope is the longest.

The General Formula:—

The factor of safety $= y_2 = \frac{a\,y_1}{a+b}$, whence $\frac{b}{a} = \frac{y_1 - y_2}{y_2}$ but, $\frac{b}{a} = \frac{x \cdot s}{c.} \times \frac{y_1}{m.s.B}$, where "m" = fraction of wire section remaining in a worn rope. Combing these equations, there results the general formula:—

$$x = \frac{y_1 - y_2}{y_1 y_2} m.C.B = \frac{y_1 - y_2}{y_1 y_2}.D$$

where $D = m.C.B$.

If $y_1 = 8$ and $y_2 = 4{\cdot}5$ $x =$ (very nearly) $\frac{1}{10}D$.

If $y_1 = 9$ and $y_2 = 4{\cdot}5$ $x = \frac{1}{9}D$.

VALUES OF D.

m.	B × C.					
	108×600.	108×500.	108×400.	120×600.	120×500.	120×400.
1·0	64,800	54,000	43,200	72,000	60,000	48,000
·9	58,320	48,600	38,880	64,800	54,000	43,200
·8	51,840	43,200	34,560	57,600	48,000	38,400
·7	45,360	37,800	30,240	50,400	42,000	33,600
·6	38,880	32,400	25,920	43,200	36,000	28,800

The value of C depends on the weight per ft. of the rope. As the rope decreases in weight owing to abrasion or corrosion, or stretching, so does C increase.

ROGER PRICE (Member) contributed the following to the discussion on the foregoing paper. He said:

In order that I might the better discuss Mr. Vaughan's valuable paper, I have re-read his former classic communication on the subject of "The Effect of Kinetic Shocks on Winding Ropes in Vertical Shafts," extracts from which appear in an abridged and revised form in the appendix to his present paper.

As far as I am able to judge, the author's conclusions arrived at by that investigation are in the main correct, and especially the one relating to the "critical depth."

In his present paper the author has furnished a very lucid investigation of the subject of "The Factor of Safety of Wire Ropes," and he is to be congratulated on his endeavours to arrive at simple formulae. In the writer's opinion, he has perhaps erred somewhat in this direction.

The problem is not in itself an easy one, and it seems almost inevitable that more or less complicated mathematics must attend its *adequate* solution. The first point which appears to be open to criticism is the author's method of estimating the breaking strength of a rope. The writer doubts whether the factor of 90 per cent., by which he multiplies the actual wire strength, in order to arrive at the strength when laid up, is a fair average.

At a later stage in this contribution the writer introduces a set of formulae for comparison with the author's, in which the "useful steel" factor is used. This also is more or less empirical, but the writer claims that it is fairly correct, as the result of many years of use, in comparison with the breaking strengths of wire ropes, both as published by makers, and as records of actual tests.

Referring to Diagram 2, the author proposes to allow the rope denoted by $y_1 = 8$ to go down further, to depths where y_2 is 5, or even 4·5. This proposal appears at first to afford a very desirable relief in the case of deep shafts, but is evidently in conflict with the proposed modification of the regulation, given further on, which proposes to limit the "capacity factor" to a minimum of 8, and the "safety factor" to a minimum of 4·5.

In Diagram 1, and in several other places it is clearly shown that these two conditions occur simultaneously only at a depth of 5,250 feet, and that at, for instance, the "critical depth" on curve II.C. (3,111 ft.) the "capacity factor" corresponding to a minimum safety factor of 4·5 is only 6·075 (for the value $B = 108$).

The writer's opinion is that any proposed relief on the lines indicated by the author, which does not provide for shafts as shallow as, say, 3,000 feet, will have no practical value. By way of a *reductio ad absurdum*, it is evident by applying Equation 1, in its modified form, that the value of "B," to conform with the proposed regulation in a 3,000 ft. shaft, is 61·7 tons, corresponding to a breaking stress of the wire of only 68·5 tons. Using 120-ton steel with a "capacity factor" of 8, the value of y_2 then becomes 5·538, or very little better than common existing practice.

To revert to the author's method of estimating the strength of a rope:—the writer, in his opening remarks, questioned the factor 90 per cent., which seems to him rather high, in comparison with the factor $x = \cdot 34\ d^2$, which, in a previous communication, has been called the "useful area of steel" in a rope.

The aggregate steel contents of seven ordinary six-strand ropes in terms of "d," the diameter of the rope, are tabulated below:—

Construction of Rope.

6/7	6/13	6/15	6/16
$\cdot 392d^2$	$\cdot 388d^2$	$\cdot 388d^2$	$\cdot 395d^2$

6/17	6/18	6/19
$\cdot 401d^2$	$\cdot 404d^2$	$\cdot 407d^2$

Neglecting core wires and making the usual commercial reduction for laying up, the very convenient *approximate average* factor $x = \cdot 34d^2$ is deduced for all the above constructions, and it is about 85 per cent. of the aggregate steel contents. As a matter of fact, this varies, for different constructions, between $\cdot 33d^2$ and $\cdot 345d^2$, and for close calculations the writer uses the exact value in each case.

The following investigation and resultant formulae are submitted as probably giving results somewhat closer to accuracy than those given by the author of the paper. In doing so, the writer has altered some of the symbols, where possible, to conform

with the author's, and thus to facilitate comparison:—

Let y_1 = Capacity factor of rope.
,, y_2 = Safety factor of rope.
,, f = Actual breaking stress of steel used in the rope, in tons.
,, d = Diameter of the rope, in inches.
,, x = Useful area of steel in the rope = $\cdot 34d^2$ sq. ins.
,, w = Weight of rope in lbs. per ft. = $1 \cdot 64d^2$.
,, L = Length of suspended rope, in ft.
,, W = The weight of loaded skip or cage, in tons.

The breaking strength of the rope is xf.
The weight of the loaded skip or cage (W) is $\frac{xf}{y_1}$.
the weight of suspended rope is $\frac{wL}{2,000}$.

Then we have:—

$$y_2 = \frac{\text{Breaking strength of rope}}{\text{Weight of loaded skip plus weight of suspended rope.}}$$

$$= \frac{xf}{\frac{xf}{y_1} + \frac{wL}{2000}}$$

$$= \frac{2000\, y_1\, xf}{2000xf + wL\, y_1},$$

hence:—

$$L = \frac{2000xf\,(y_1 - y_2)}{w\,(y_1\, y_2)}.$$

Substitute values of "w" and "x," then:—

$$L = \frac{2000\,(\cdot 34d^2 f)\,(y_1 - y_2)}{1 \cdot 64d^2\,(y_1\, y_2)} \qquad (1)$$

$$= \frac{414\ 6\ f\,(y_1 - y_2)}{y_1\, y_2},$$

$$\text{say } \frac{415f\,(y_1 - y_2)}{y_1\, y_2}. \qquad (2)$$

This is comparable with the author's modified equation (1),

$$x = \frac{500B\,(y_1 - y_2)}{y_1\, y_2}$$

in which "B" is the same thing as ·9f and "x" the same as "L," and which would then become:—

$$L = \frac{450\ f\,(y_1 - y_2)}{y_1\, y_2}$$

It will be seen that the constants 450 and 415 are approximately proportional to the factors 90 per cent. and $\cdot 34d^2$, referred to above.

The above formulae (1) and (2) are correct for a new rope, but in estimating the strength of a worn and otherwise deteriorated rope the factor "x" and "w" must be suitably reduced.

Assuming the case named in the paper, viz., a reduction in strength of 1/3, "x" will have to be reduced from $\cdot 34d^2$ to, say, $226d^2$. The unit weight "w" may, perhaps, be considered as reduced to some such value as $1 \cdot 5d^2$. The writer has not closely investigated this value, and it is advanced *tentatively* and subject to correction. It should also be remembered that the reduction in value of "x," in most cases, must not be considered as *wholly* due to reduction in cross-sectional area of steel, and therefore the reduction of "w" is not necessarily commensurate with that of "x," and again it must be remembered that there is a small but indeterminate compensation due to accretions of grease and other foreign matter on the rope. It is evident that if the values of "w" and "x" were reduced in exactly the same proportion the resultant formula for the worn rope would be exactly the same as (2).

The formula for the worn rope will then probably take some form such as:—

$$L = \frac{2,000\,(\cdot 226d^2 f)\,(y_1 - y_2)}{1 \cdot 5d^2\,(y_1\, y_2)} \qquad (3)$$

$$L = \frac{300\ f\,(y_1 - y_2)}{y_1\, y_2} \qquad (4)$$

Note.—"d" still denotes the original diameter of the rope.

Comparing formula (4), as before, with the author's formula (1), it is seen that the resulting values of "L" (the author's "x") will be 66·6 per cent. of those obtained by the author's formula (1), or 72 per cent. of those obtained by the writer's formula (2).

These somewhat startling results lead the writer to surmise that his tentative estimate of the value of "w" in a worn rope may be too high. It may possibly have, for instance, some such average value as $1 \cdot 25d^2$, in which case formula (4) would become:

$$L=\frac{360\,f\,(y_1-y_2)}{y_1\,y_2} \qquad (5)$$

giving values of L 80 per cent. and 86·7 per cent. of those obtained by the author's formula (1) and the writer's formula (4) respectively. This question of the changes during the life of a rope in the relative values of " w " and " x " appears to have a very important bearing on the problem, and the above tentative formulae relating to it are advanced in the hope of eliciting useful data, which could be easily obtained by weighing a number of specimens of worn ropes, and comparing the results with their breaking strengths. In this way it should be possible to arrive at an approximately correct single co-efficient or set of co-efficient for various constructions of rope.

It will be observed that by the use of the writer's formula (2) the " critical depth " Lc is also reduced as compared with that obtained by the use of the author's modified formula (III.), thus:

$$\frac{wL_c}{2{,}000}=\frac{xf}{y_1}(\cdot 35)$$

hence:

$$\frac{1\cdot 64d^2L_c}{2{,}000}=\frac{\cdot 34d^2f}{y_1}\,(\cdot 35)$$

and:

$$L_c=\frac{145\,f}{y_1} \qquad (6)$$

The author's equation (III) is:

$$x=\frac{175\,B}{y_1}$$

or, using the writer's symbols:

$$L_c=\frac{175\,(\cdot 9\,f)}{y_1}$$

$$L_c=\frac{157\cdot 5\,f}{y_1}$$

The critical depths obtained by the writer's formula (6) will therefore be $\frac{145\times 100}{157\cdot 5}$ = 92% of those obtained by the author.

It is clear that the writer's tentative formula (4) and (5) must *not* be used for the " critical depth." This must be determined for the new rope, and for that *only*.

For comparison with the figures appearing in the table at the top of the next page the following may be of interest.

The shallowest depths for which the author's proposed new regulation would apply, with the above grades of steel, are as follows:—

Five-ton.—By the writer's formula (4):

$$L=\frac{300\,f\,(y_1-y_2)}{y_1\,y_2}=4{,}115\text{ ft,}$$

By the writer's formula (5):—

$$L=\frac{4{,}115\times 360}{300}=4{,}983\text{ ft.}$$

By the author's formula (1):—

$$L=\frac{4{,}115\times 450}{300}=6{,}172\text{ ft.}$$

Three-ton.—In this case the three corresponding figures are 4,174 ft., 5,008 ft., and 6,265 ft.

R. B. Greer (Member) recalled a paper which he had read, and which he thought had contributed to the knowledge that very high breaking strains in wire ropes were a disadvantage. With regard to deep level winding, there should be a little relief given from the six factor of safety, including the weight of the rope. He realised that the elasticity of the rope nearly counterbalanced the weight. His experience of seventeen years in supplying wire ropes had led him to believe that a rope nearly always broke near the skip, showing that the extra margin of safety which was allowed for the weight of the rope was not needed.

The few serious accidents in the early days of the Rand occurred when the rope was nearly up to the top of the shaft. In wire ropes the break, when it did occur, was mostly near the bridle.

As to kinetic shock, did Mr. Vaughan mean a recurring kinetic shock, or the one shock when an accident occurred? With the modern electric winder, with power transmitted from a turbine engine, the recurring shock of the reciprocating engine could be disregarded in the deeper shafts of the Rand. The wave shock of a sudden accident did not mean anything at all because the first wave was so much less than the original shock, and each successive shock went dying down, and did not need to be taken into one's calculations.

If they insisted on their present factors of safety at the end of the rope, and added only one-third of the weight of the rope, they would arrive at a workable formula.

PARTICULARS OF ROPES ON THE FIVE-TON HOISTS, DEEP SHAFT, CINDERELLA CONS. G.M., LTD.

Hoist Capacity.	d Ins.	w Lbs.	f Tons.	B.W. Tons.	y_1	y_2	L Feet.	W Tons.	$\frac{wL}{2000}$ Tons.	$W+\frac{wL}{2000}$ Tons.
5 tons ..	$1\frac{3}{8}$	3·19	141	97	11·75	6·5	4,200	8·25	6·7	14·95
	If "B.W." reduced by $\frac{1}{3}$ and "w" reduced by $\frac{1}{10}$		—	65	7·88	4·5	4,200	8·27	6·03	14·28
	If reduction of "w" neglected		—	65	7·88	4·34	4,200	8·25	6·7	14·95
3 tons ..	$1\frac{1}{4}$	2·08	143	65·3	11·87	6·6	4,200	5·5	4·4	9·9
	If "B.W." reduced by $\frac{1}{3}$ and "w" reduced by $\frac{1}{10}$		—	43·5	7·9	4·6	4,200	5·5	3·96	9·46
	If reduction of "w" neglected		—	43·5	7·9	4·4	4,200	5·5	4·4	9·9

One must come back to the result of experience. In a depth of 6,000 feet they would never get a curve which would be perfectly correct, but he thought it would be fairly correct if they worked on a factor margin of six for the weight at the end of the rope and a third for the weight of the rope.

J. W. KIRKLAND (Member) said that a long elastic rope would be subjected to a great deal more strain near the point where it was fixed—as in the bridle of the skip—than anywhere else. Travelling waves must bank up where they are brought to a standstill, and the increase may amount to double the ordinary stress. This was as true in mechanics as it was in electricity. They had all read in the papers within the last year or two of the examination of steel castings by means of X-rays for the determination of flaws. He asked the author of the paper if it was not possible, when ropes were suspected or when they were worn, for some X-ray examination to be made to determine the condition of the inside, which was otherwise inaccessible. Since it was done with steel castings, why not do it with wire ropes?

W. MARTIN EPTON (Past President) said that the subject of Mr. Vaughan's paper had been discussed many times, but this was the first time that any finality had been arrived at with regard to the safety factor in ropes. Two points had occurred to him. The first was in reference to Mr. Price's criticism regarding the laying-up factor in ropes. If his memory was correct the difference between the strength of the rope and the aggregate of its wires varied with different constructions from something between 4·5 per cent. to about 16 per cent., so that Mr. Vaughan's 10 per cent. very largely depended on the construction of the rope, and presumably there would be no difficulty in altering Mr. Vaughan's figures to suit the various rope constructors.

Mr. Kirkland had suggested that X-rays might be used in determining the condition of the inside of the worn rope, and certainly the speaker had a great amount of sympathy with the resident engineer who was responsible for the assessing of the strength of the rope, and if the X-rays could be used for this purpose it might to a certain extent ease the responsibility of the resident engineer, who was responsible for the safety of thousands of lives. The engineer's trouble began as the rope began to wear and other deleterious effects occurred, such as corrosion, etc. There were certain guides as to the conditions of the wire rope, which the engineer now had, but which were not realised some fifteen or twenty years ago. To-day, however, the

majority of them had a large experience, and were able from certain indications to more or less assess the strength of an old rope. It was comparatively easy for an experienced man to assess the strength of the worn rope, provided the wear was entirely on the outside, and there was no interior corrosion.

Regarding corrosion, a fairly certain guide was to take the weight off the rope, and if the wires then stood proud, one might be certain that the internal corrosion had taken place. Also if it was found that the diameter of the rope was considerably reduced, more than was accounted for by the outside wear, then it was also fairly certain that there was internal corrosion.

The speaker did not know if he could say much more at present with regard to the paper, but it appeared that Mr. Vaughan had not only rendered great assistance to the local mining industry, but also the result of his work could be felt in every mining centre in the world. Mr. Vaughan had probably done more for the safety of winding than any other man. The American Commissioners had been for nine years considering the subject, but had arrived nowhere. The English Commissioners had vaguely said that such and such had been the case, and might be permitted, but had never given any difinite line to work on. Mr. Vaughan, that night, had given them definite conclusions. The speaker remembered that when the Safety Catch Commission was sitting Mr. Vaughan had stuck to the factor of safety of six, because he was not certain whether he could with safety reduce that factor. Now, however, after ten years' further investigations he evidently felt certain in his mind. To-night the members had had the privilege of listening to a paper that would probably be a classic with regard to wire ropes, and live for all time, and the speaker would like to add his thanks to Mr. Vaughan for the valuable work he had done.

E. J. Laschinger (Past President) said he desired to associate himself with the remarks of Mr. Epton with regard to Mr. Vaughan's paper and the other contributions which he had in the past given to this Institution on the subject. He thought it had been admitted by practically all engineers who had studied the problem that the factor of safety of six for a very deep-winding proposition was too high, and he thought that Mr. Vaughan in introducing a new factor to assess the safety of a rope on capacity had made a very distinct advance with regard to regulations which might be framed in this or any other country where mining enginers and those associated with them had to design installations. Mr. Roger Price, in his discussion, had brought up particularly one phase of the subject which should receive very special consideration. He thought Mr. Vaughan realised it, but did not mention it in his paper. The solution he suggested with regard to wire ropes was, of course, that the capacity factor proposed by Mr. Vaughan should be based upon the tests made from time to time on sections of the rope near the skip so as to introduce not the safety factor, which was at present the governing thing, but the capacity factor. With regard to the strength of the rope, he thought some of the older members might bear him out in recollections of discussions which had taken place before this Institution and their kindred society, the Chemical, Metallurgical and Mining Society, concerning the deterioration due to the action of gases in the shaft. The diminution in strength of a rope did not depend entirely upon the reduction of the area. Naturally, if one-eighth of the working area of steel wore away, then only seven-eighths strength was left. But they had two other factors to consider. One was the fatigue from age which occurred, and, secondly, there was the diminution of strength due to the action of gases upon the rope. It was very well known that certain gases, more especially hydrogen, which were evolved by the action of acid and mine air upon steel, were absorbed, and that had a tremendous influence in diminishing the strength of the steel. It was also well known, and had been demonstrated by experiment, that if those gases were again driven off by subjecting the steel to a certain temperature, the steel returned to the original strength. Therefore, in any calculations which might be made to determine whether a rope, after it had once been put on as new, was perfectly safe, allowance should be made for deterioration due to wear and to natural fatigue, and they had, therefore, to fall back upon the final test, which was made at certain periods, according to regulations. They could then determine whether that rope was safe, not by going upon the fac-

tor of safety, but the capacity factor which Mr. Vaughan had put forward that night as a more reasonable solution for winding ropes in deep-level shafts. Mr. Price had criticised Mr. Vaughan's paper, because, he said, according to his own calculations that rope was only fit for shafts of a certain maximum depth; but surely if a rope was safe for a shaft of 5,000 feet, why worry about the same rope for a depth of 2,000 feet?

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

PAPER BY J. A. VAUGHAN (PAST PRESIDENT).

Discussion.

G. M. CLARK (President) said: Before asking members to continue the discussion on Mr. Vaughan's paper, I would like to say a few words upon it myself. Perhaps not so much on this paper as on papers of this kind. When an observation is made it is not, as is so often stated, an observation of *some thing*, nor may it even be an observation of the relation of two things to each other. We cannot observe one thing any more than we can see a point as Euclid defined it, nor, unfortunately, can we often observe the simple relation of two things to each other. Natural relationship is always complicated, and the relationships that we observe are often not real, but spurious, having been brought about by some unseen connecting link, such as a mere synchronism in time of happening not unfrequently provides. If, however, we are fortunate enough to find the relation between two things, or to think that we have done so, we can tabulate or chart our experience for future reference. We soon begin to find as we collect such data, such, for instance, as the depth of shaft and diameter of rope, that the first relationship has been discovered under one set of conditions that are perhaps not repeatable. More data is collected and our tables become voluminous or our charts become covered with many spots like stellar constellations. From the chart we can always obtain two results straight away. By sufficiently compressing one dimension the points will all lie on a vertical line, whilst by a suitable compression of the other dimension the points will all lie on a horizontal line. By this mathematical treatment we thus immediately obtain two results that do not support each other, for the lines expressing them are at right angles. The thing that has been left out is the sense of proportion that comes from experience either in dealing with the relationships originally or in their pseudo-mathematical treatment afterwards. A person of experience in a particular class of observations will choose the scales of the diagram so as to exhibit fairly well the real relationship to the best of his knowledge; whilst an inexperienced person will either mask the real variations by the smallness of the scale, or fall into the opposite error and show curves full of weird kinks and turns, through choosing exaggerated scales greater than the accuracy of the original observations will bear. The curve given by an experienced person is a form of shorthand statement of the evidence in the case as he sees it, sorts it and assimilates it. Instead of drawing a curve, a mathematical formula may be used for this shorthand statement. Such an expression then forms a sort of datum line from which to think, or perhaps, since a datum line is usually taken either above or below the works to be carried out, a better simile is that the curve or mathematical expression forms a thread runnning through the path of our experiences. By good luck such a thread may be along the centre of the path, and by bad luck it may at times cut outside the path altogether. Such a thread may lie in many positions of equal validity, and may either be of a coarse texture, such as Mr. Vaughan has left it, or spun finer by a more minute examination of certain constants, as Mr. Roger Price would have it. Useful as such a thread is as a guide, it does not often tell us what we are all most anxious to know, and that is the width of the road down which we have to travel. Experience alone can give us this. True, there are relationships behind which there is a sufficient body of evidence to make us feel sure that the path is a narrow one and very nearly co-incident with the thread of its mathematical expression. But the subject under discussion is scarcely one of these, for the concomitant variables brought about by use—the effects of corrosion, for instance, have already been mentioned—are of more importance during service than the variables that are introduceable into the discussion of the problem before service.

Obtaining numerical results from such formulae is such a facile and dangerous performance that one sometimes wishes that arithmetic had become a lost art or was only taught, if it must be kept alive, to a selected few who have had sufficient experience in the field to collect their data and sufficient mathematical training to collate them. If this were done we might treat problems more soundly, though possibly in duller fashion, for it surely adds to our gaiety to see problems such as the flow of water in a channel worked out from formulae given for uniform velocity when the experience is that of a flow under far from uniform acceleration. Easier labour problems, such as the building of a wall by eight men working twelve hours a day would not lead to the result that twelve men are required for eight hours a day, the right answer is probably about 10 when arithmetic is suppressed and experience of fatigue is understood in its place.

As I said at last monthly meeting in moving a vote of thanks to the author, I greatly appreciate the elegant treatment he has given this problem, and the neat manner in which he has drawn the thread. I have only joined in the discussion to point out the dangers of such a thread unless fortified by experience, and trust that those who have the necessary experience will give it for the benefit of the Institution. I hope that the discussion will be pursued as vigorously as it was last month.

A. E. DU PASQUIER (Member) said: I have nothing but admiration for the author's paper. In the last ten years I have had to interest myself to a considerable extent in the question of winding ropes, and have never seen the subject handled in a more original or enlightening way. I am not one of those who think the winding problem in South Africa has in the past been handicapped to any extent by rope limitations or Government regulations. The tendency of the future, however, is beyond doubt in the direction of heavier loads at reduced winding speeds, and I welcome the author's proposal to reduce y_2 when outside what he calls the danger zone, as it enables new conditions to be met, and unquestionably makes for reduced cost and greater efficiency without any real sacrifice in safety.

The maximum conditions likely to be encountered on the Reef are 30 tons of rock and skip from a depth of 6,000 feet, which a 2¼-inch diameter rope 154 ton tensile steel will handle with a factor of safety, y_2 of 4·5; such a rope is of course expensive, but is capable of giving quite satisfactory results in practice in a vertical shaft, and with due regard to rope leads, drum diameters, etc.

I have some figures of a 2-inch rope 6/37/·095 tensile 147·2, winding 16,000 lbs. of rock from 4,500 feet, which has been in operation some thirty months, and is good for still longer life. As further illustrating the reliable performance of ropes of this class, I may mention the case of a winding engine in which I was largely interested at Home, which was designed to raise 13·4 tons of coal from a depth of 2,250 feet—the actual weight at the end of the rope was 32 tons, y_1 and y_2 being 7·875 and 6·675 respectively.

When tubs and cages are employed, the author's proposals are the more valuable. As an example, if skips could have been used in the coal winding engine mentioned, the 2¼-inch rope could have been reduced to 1⅞-inch diameter, or steel of a lower tensile value used.

Another appreciable advantage of the skip method of winding is that shocks, coming under the author's categories II and III, are largely or entirely avoided.

A further advantage of the reduced factor of safety is, of course, that it permits of using a lower tensile steel without unduly increasing the diameter of the rope.

While I submit that steel of the highest obtainable breaking stress can be used with satisfactory results in a vertical shaft under good conditions, this is certainly not the case in vertical shafts where the rope conditions are bad, excessive angle of lead, coiling on small drums in three or four layers, or generally any excessive number of bends.

I have recently had an opportunity of looking at some very accurately kept records of rope life in a compound shaft, extending over the last four or five years. The records distinctly show that the best results were obtained from ropes of large diameter wires and moderate tensiles.

The most satisfactory rope in this particular case was a 1¼-in. diameter 6/7·140/·142, tensile 123·2, such ropes having an average

life, apart from accident, of approximately 350 days, as against perhaps 150 days for ropes composed of smaller wires and higher tensile steel.

It is interesting to note that with this 6/7 rope the ratio D—d works out at 723, which is rather on the low side. The use of, say, a No. 9 s.w.g. wire, ·144-inch diameter, which is as large as one would want to go, need not involve any drum restrictions, as with a ratio of D—d 1,000, the minimum diameter of a drum would be 12 feet. This is not an unreasonable dimension for a winding engine of any size, and at a rope speed of 2,000 feet per minute as a lower limit, the drum speed is 53 r.p.m., which can still be either comfortably geared to a standard speed motor, or coupled direct if the size of the equipment would warrant it.

Dealing now with the question of using steels of a lower ultimate stress, it must be conceded that, other things being equal, the lower tensile steel rope will have the longer life, which is a distinct advantage here, where there is no Government regulation as to the time a rope may continue in service.

I am advised that the pre-war price difference for the same size rope at 98 tons and 154 tons per square inch was as much as 70 to 80 per cent.; one could therefore, if there were no other limitations, increase the sectional area of the rope in proportion to the tensile, and still get a cheaper rope. Of course, other factors come in, drum dimensions and so on, and in the past an exaggerated idea of the necessary factor of safety was an influence in favour of high tensiles. Also, I believe, the idea is prevalent that the lighter the rope the less the power consumption per wind. It is of course less to some extent, but usually quite inappreciably, as the extra weight of the rope when winding in balance only slightly increases the inertia of the system, and consequently the power consumed in the accelerating period of the wind, which to a large extent may be recovered in the latter portion of the wind. To some small extent the friction losses may be also increased, but it would be an exceptional case when the increased weight of the rope would have any material bearing on the overall efficiency of the wind.

The point I am trying to make, in perhaps a rather roundabout way, is that the proper use to make of the author's proposal for reducing y_2 is not primarily to cut rope diameters, but to use steel drawn to a lower ultimate breaking stress, and only to utilise the higher values when up against rope diameter limitations.

I notice that the author omits from his calculations any stresses due to acceleration bending or rope distortion, and agree with him that in the majority of cases it is not necessary to take these factors into account. With the modern tendency to restrict winding speeds, previously mentioned, the accelerating stress will usually be less than 1 per cent. of the dead load, including rope, and whereas at one time I endeavoured to show that the bending stresses might amount to approximately 30 per cent. of the total load on the rope, with a ratio D—d 1,000, the fact that rope distress is usually manifested in that part of the rope which never coils on the drum, has led me to think that the recognised method of calculating these stresses is not in accordance with actual practical results.

In theory this stress may vary from a maximum of 30,000 lbs. per square inch for a straight wire to 10,000 lbs. for a rope with this particular ratio, and my former method of calculating the factor of safety was

$$\frac{\text{tensile of steel} - \text{bending stress} \times \text{area of steel} \times K}{\text{dead load} + \text{accelerating stress.}}$$

K being of course a constant, depending on the make-up of the rope, and varying according to construction from ·80 to ·95, but for the reasons stated above, I agree with the author that a ratio of D—d 1,000 bending stress can be neglected.

With the author's curves and formulæ rope calculations become a very simple matter, y_1 giving immediately the breaking load of the necessary rope, and having decided on any suitable figure for B,

$$\frac{y_1 \times \text{the load}}{B}$$

gives at once the sectional area of the rope regardless of depth, and the actual y_2 for any given depth can be conveniently calculated from the author's equation:

$$x = \frac{y_1 - y_2}{y_1 y_2} 500\ B.$$

From the formula you can determine x or y_1 or y_2, but you must not try to deter-

mine B, as, for example, for 6,000 feet and factors of safety of 9 and 4.5 (it is essential to take points on the same curve) B = 108 tons per square inch, which would result in much too large a rope if the load " a " is at all on the high side.

I would like to ask the author why he proposes a y_1 of 12, when his calculations seemed to show $y_1 = 9$ to be sufficient, and why he finds it desirable to have a margin of 33-1/3 per cent. Also, I should be glad if he would explain what is really meant by allowing the rope to wear down one-third.

y_1 12 allows for a 25 per cent. reduction in area before y_1 9 is reached; this would correspond to a 12½ per cent. reduction in the diameter of the individual wires or in the circumference of the rope, assuming uniform wear and neglecting the hemp core, if there is one. A 2-inch rope could therefore be reduced in circumference from approximately 6-9/32 inch to 5½ inch, if there were no broken wires—the effect of considering the hemp core would be to exaggerate the above difference. I imagine it would be many years before a rope would shrink with fair wear and tear to such an extent, and suggest that such a rargin is not necessary. It does not follow that flattening is accompanied with appreciable loss of section, and with moderate tensile steels there is, I think, no doubt that during the life of the rope there may be an actual gain in breaking stress, due to the gradual hardening of the wires, due to alterations of stress and release of stress.

Broken wires are another matter, but I do not think any one would care to run a rope in which broken wires accounted for a 25 per cent. reduction in effective area. The point I wish to make is that in well-laid out vertical shafts there should be no need to increase y_1 above the author's figure of 9. Whether, in compound shafts and on inclines, where the rope gets so much harder usage, it is worth while for economic considerations to put on larger ropes, is a matter for individual experience.

It would be most instructive if the author in reply could give us some actual figures as to the breaking loads of ropes, when new and when discarded, and the actual loss of sectional area by measurement. Generally, it would add to the value of the paper if either during the discussion or in the author's reply some actual figures of rope performance on the Reef would be given.

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

PAPER BY J. A. VAUGHAN (Past President).

Continued Discussion.

A. C. WHITTOME (Past President) said: Mr. Vaughan's paper is so highly valuable, and deals with a subject of such great importance, that it is to be deplored that our Council did not decide to hold a special meeting at which it could have been presented and discussed.

At the present time it is of especial importance to lengthen the service of ropes in deep level shafts, if it is possible to do so with safety; it is, therefore, especially to be regretted that so little progress has been made during the past two months in determining whether the engineering profession as a whole is satisfied that the proposals made by the author are sound. It is hardly to be expected that the Mines and Works Regulations will be changed in the direction of lowering the minimum permitted factor of safety of ropes unless users show that they are convinced that the reduction is compatible with the safety of persons and property. The fact that the discussion of the subject has been originated by the Chief Inspector of Machinery is an extraordinary indication of the sympathetic consideration extended by the Mines Department to the difficulties experienced by the mining community. Probably this is an almost unique example of its kind in mining history.

One could, of course, consider that the small amount of discussion of the paper hitherto contributed is an indication that ropes are so easily and cheaply procured by the mines that the reduction of the minimum factor of safety from 6 to 4·5 is a matter of little moment, were it not for the absolute knowledge that such is not the case. Far from it being a fact, it is almost certain that before the termination of the War it will be so difficult to procure ropes for deep level mines that it may even become necessary to run with a minimum factor of safety of below 4·5 if certain hoists are to be used for the transport of persons.

I desire to support the author of the paper in certain directions, particularly with regard to his equations for the percentage that the ultimate strength of the wire as laid up in the ropes bears to the original value of the wire before laying up, and the weight of rope per foot of length.

It is, of course, impossible to arrive at any empirical formulæ which will give absolutely accurate results for all sets of conditions. A rope of 6/6/1 construction must, of necessity, differ from one of the same diameter, but of, say, 6/18/12/6/1 construction in regard to sectional wire area and weight, therefore the very useful formula,

$$\text{Wire sectional area} = \cdot 4\ D^2.$$

can only approximate to accuracy (though the approximation is a very close one) in the two cases. As there are very numerous classes of construction of ropes, and each class would need a separate constant for each rope diameter, if absolute accuracy was desired, it becomes impossible to write a formula which gives anything but a very close approximation. This is given by the constant mentioned above.

The deduction of 10 per cent. from the original strength of the wire to compensate for the reduction in value, due to laying it up into the rope, is also sound. In No. 9, Vol. III of the Proceedings of the Transvaal Institute of Mechanical Engineers, page 246, Mr. J. A. Vaughan and Mr. Martin Epton gave the results of forty typical tests on new wire ropes. The test strengths of the various ropes were from 1·9 per cent. to 17·6 per cent. below the aggregate strength of the wires in the individual ropes. In thirteen instances out of forty the percentage difference was above 11 per cent.; in eighteen instances below 9 per cent.; nine were between 9 per cent. and 11 per cent. difference; six were below 5 per cent. difference. I understand, though I am open to correction on this point, that fifteen years' experience in the testing of ropes in the Mines Department Mechanical Laboratory has convinced the Department that the 10 per cent. reduction used by the author is a satisfactory figure to take, being amply high to cover all but a few exceptional ropes (and these latter would be of poor quality in any case), yet as low as it is safe to take, even though there are numbers of instances where the aggregate wire strength has only to be reduced by 4 or 5 per cent. to arrive at the rope strength.

In this connection it is perhaps well to point out that a test carried out on a 3ft. or 6ft. sample of rope does not necessarily give the exact conditions throughout the whole length of the rope, even when it is new—much less when worn. I have seen in the Mines Department Laboratory a number of samples of new wire cut in succession from the same coil and tested both for tension and in the fatigue testing machine; no two samples gave exactly the same results. This being so with wire from the same coil, it becomes an impossibility for the 42 to 100 or 200 wires in a 5,000 ft. rope to be more than approximately of the same value. When the wires are laid up into the strands they are not laid up with exactly the same tension on all the wires, therefore some wires take more than their exact proportion of the load on the strand; the strands also are not laid up with exactly the same tension on each of them, therefore each strand does not take its exact proportion of the load on the rope. That these statements are correct is proved by the fact that, when a sample of new rope is tested to destruction, first of all one or more overloaded wires break, then one or more overloaded strands. I believe that it is very unusual for more than three strands to break at the same time, and more than four breaking at once is unknown, whereas it is a very common occurrence for a single strand to break with an appreciably lower load on the rope than is required to rupture a further two or three strands, in spite of the fact that when one strand breaks the remaining five have at once to carry over 20 per cent. increase of stress. It is obvious that if all the wires were of exactly the same value, and there were no differences in tension on the wires (or strands) when laid up, the whole of the wires in the rope would break at the same moment. The 10 per cent. allowance covers these various factors.

Mr. Price states (pages 105-6 of the November, 1917, *Journal*) that he thinks Mr. Vaughan's assumed values for breaking strengths, etc., are "rather high." For the past fifteen years I have taken the gross wire area of a rope to equal ·4 D^2, the net strength of the laid-up wires to be 90 per cent. of the wire strength, and the weight in lbs. per foot to equal 1·6 diameter in inches squared (or alternatively 4 lbs. per foot per square inch of wire area). I am

sure that 1·6 is a better constant to take than Mr. Price's 1·64. For the past five or six years I have always reckoned the full area of wire as being useful. I think that the practice of deducting the core wire from each strand is out of date, owing to the almost universal use of hard core-wires, any slightly lower value for this wire being covered by the 10 per cent. allowance. Therefore my standard formula becomes:

Breaking strength = ·4 D^2 × ·9 gross wire value,
= ·36 D^2 × gross wire value,

or ·36 D^2, as against the ·34 D^2 advocated by Mr. Price. Undoubtedly Mr. Price could find a few ropes the tests of which agreed exactly with his constant of ·34. I know I should find it impossible to prove that ·36 is exactly correct in all cases. It is, however, unfortunate for Mr. Price that, whilst he says the 90 per cent. is too high, the cases of the only two ropes he instances show that it is too low. On page 108 of the *Journal* Mr. Price gives details of two ropes, one 1⅜ inch, the other 1¼ inch diameter. Using the formula B.S. = ·36 D^2 × gross wire value, the breaking loads become, for 1⅜ inch diameter, 95·96 tons, as against the actual breaking load of 97 tons, and, for the 1¼ inch diameter rope 65·15, as against 65·3 tons. Substituting Mr. Price's constant of ·34, the loads would be 90·63 and 61·53 tons respectively, thus proving that the ·34 constant is considerably (about 7 per cent.) too low for these two ropes. The fact that the diameters of ropes, as specified by makers, is only a "nominal" one makes it impossible to more than approximate to the rope value when considering any problem in which there is not an actually new rope to take into account. The appended list, Table I, is abstracted from the catalogue of a prominent rope maker.

When a 1⅛ inch nominal diameter rope can measure from 1·114 inch to 1·153 inch it becomes necessary to take *actual* instead of *listed* diameter into account.

Table II is of especial interest in this connection, as it indicates that, when the weight per foot differs to any material extent from 1·6 D^2, it is because of the fact that the actual diameter differs considerably from the nominal, and not because of any marked incorrectness in the formula constant.

At first sight it would appear that the formula for breaking loads (·36 D^2 × gross wire strength) gives too great a load. By comparing the columns it will be seen that the "actual" breaking loads, as given in the catalogue (which is that of a renowned British rope-maker), are consistently about 5 per cent. below the calculated ones given in the adjoining column. The explanation is that these particular manufacturers, at the time their catalogue was issued, invariably used soft core wires in their rope strands; indeed, they—and other makers—still do so to a great extent for home use. The following extract from a letter written in 1914 to a Johannesburg correspondent explains the matter fully:—

> "Unless specially ordered, we have always used mild steel for our core wires, as we have held the view that in some respects it is better for the rope. At the same time, if core wires of the same quality as the other wires of the rope are used, it will undoubtedly increase the breaking strain. You will understand that in a 6/7 rope the core wires (six in number) represent one-seventh of the metallic area of the rope, or over 14 per cent. If these core wires were made of hard steel the breaking strain of the rope would, of course, not be increased by 14 per cent., but, certainly, it would be by 5 per cent. to 7½ per cent."

Mr. Price's constant of ·34 gives rope strengths between ½ and 1 per cent. below those given by this maker for "soft core wire" ropes; whilst the constant of ·36 gives roughly 5 per cent. higher than the maker's "soft core wire" figures, and, therefore, not too high a breaking load for a "hard core wire" rope, which is the usual case, at any rate on the Rand.

It is seen that with the formula 1·6 D^2 the weights per foot length are practically uniform with those given by the makers. This constant is arrived at by multiplying the area of wire (·4 D^2) by 4 lbs. weight per square inch per foot length, as used by Mr. Vaughan (thus giving 1·6 D^2 as weight per foot).

The following formula for arriving at the required diameter of rope to raise a given load of skip and contents from a given depth may be useful to members:—

D = Diameter in inches.

Per stress = Tensile strength of wire in lbs. × ·9 divided by factor of safety at upper end.

TABLE I.

Nominal and Actual Diameters of Ropes.

	in.	in.	in.	in.	in.	in.
Nominal diameter ..	½	½	⅝	⅝	¾	⅞
Actual diameter ..	·447	·517	·596	·636	·656	·875
Nominal diameter ..	⅞	1⅛	1⅛	1⅛	1⅜	1⅜
Actual diameter ..	·895	1·114	1·133	1·153	1·352	1·392
Nominal diameter ..	2	2	2⅛	—	—	—
Actual diameter ..	1·989	2·030	2·148	—	—	—

TABLE II.

Nominal and Actual Diameters; Actual and Calculated Weights of Ropes per foot length; and Actual and Calculated Breaking Loads; the nominal and actual figures being taken from a prominent British rope-maker's catalogue, the calculated figures being arrived at from the formulæ given.

Diameters.		Weights in lbs. per foot.		Breaking Loads with 120-ton Steel Wire.	
Nominal. Inches.	Actual. Inches.	Actual. lbs.	From weight $=1{\cdot}6\ D^2$. lbs.	Actual with soft core wire. Tons.	By $=\cdot 36\ D^2 \times T.S$ with hard core wire. Tons.
½	·477	·375	·364	9·37	9·83
½	·517	·437	·428	11·00	11·54
⅝	·596	·567	·568	14·64	15·34
⅝	·636	·625	·647	16·66	17·47
¾	·656	·708	·689	17·73	18·59
¾	·755	·917	·912	23·49	24·62
⅞	·875	1·250	1·225	31·45	33·07
⅞	·895	1·291	1·282	32·95	34·60
1	·994	1·583	1·581	40·68	42·68
1⅛	1·114	1· 86	1·986	51·00	53·61
1⅛	1·133	2·125	2·054	52·83	55·45
1⅛	1·153	2·166	2·127	54·75	57·43
1¼	1·233	2·500	2·432	62·53	65·67
1¼	1·273	2·667	2·592	66·67	70·00
1⅜	1·352	3·000	2·925	75·24	78·96
1⅜	1·392	3·167	3·100	79·74	83·70
1½	1·511	3·667	3·653	94·00	98·63
1⅝	1·631	4·250	4·256	109·33	114·91
1¾	1·751	4·910	4·906	126·00	132·45
1⅞	1·900	5·833	5·831	150·00	157·43
2	1·989	6·333	6·330	162·72	170·90
2	2·030	6·580	6·593	169·32	178·02
2¼	2·148	7·375	7·366	189·84	198·89

Length = Length of rope in feet.

$$D = \sqrt{\frac{\text{Load of rock and skip in lbs.}}{(\cdot 4 \text{ per stress lbs.}) - (1 \cdot 6 \text{ length})}}$$

If the rope is used on an incline both load and length must be multiplied by the size of the incline angle.

I have used this formula for many years, and it gives absolutely accurate diameters.

If Mr. Vaughan's proposals are adopted it will be necessary to write fresh formulæ for finding the required diameter of rope for a given hauling problem. The following may be found convenient, and they have the advantage of giving exact diameters, and not approximate ones:—

(1) To find the required ultimate strength of wire of which a rope hauling from a given vertical depth must be made in order that the selected factors of safety for the two ends may be attained—

x = Length of rope in feet.
y_1 = Factor of safety at lower end of rope.
y_2 = Factor of safety at upper end of rope.
a = Supported load at lower end of rope.
f = Ult. strength in lbs. per square inch of wire used.

$$\text{Ult. strength of wire} = \frac{4x}{\cdot 9\left(\frac{1}{y_2} - \frac{1}{y_1}\right)}$$

For the given depths and factors of safety *no other wire can be used.*

(2) To find y_2, when using a given strength of wire and a given factor of safety (y_1) at lower end, for a given depth vertical shaft:

$$y_2 = \frac{1}{\frac{4x}{\cdot 9f} + \frac{1}{y_1}}$$

(3) To find y_1, when using a given strength of wire and a given factor of safety (y_2) at upper end, for a given depth vertical shaft.

$$y_1 = \frac{1}{\frac{1}{y_2} - \frac{4x}{\cdot 9f}}$$

The diameters of ropes are of course found in the ordinary way.

Say, for instance, a wire of 240,000 lbs. strength is to be used and the factor of safety at the lower end of a 4,000 feet shaft is to be 10, the lifted load of skip and rock being 20,000 lbs.

Factor of safety at top = y_2 =

$$\frac{1}{\frac{4x}{\cdot 9f} + \frac{1}{y_1}} = \frac{1}{\frac{4 \times 4{,}000}{\cdot 9 \times 240{,}000} + \frac{1}{10}}$$

y_2 = about 5·7

Diameter of rope =

$$\sqrt{\frac{\text{Load at bottom} \times y_1}{\cdot 36f.}} = \sqrt{\frac{20{,}000 \times 10}{\cdot 36 \times 240{,}000}}$$

$= \sqrt{2.31} = 1.52''$ diameter.

I am inclined to think that the modification of Mr. Vaughan's formula suggested by Mr. Price for use with worn ropes will not appeal to practical men. The examination of ropes and estimation of their deterioration is probably the most important duty of a resident engineer. Once the engineer relies upon a formula for worn ropes he is courting disaster. His proper method of work is to take all details as to original strength from the test certificates and to rely upon his own investigations and measurements to arrive at the amount of that original strength which has been lost during the life of the rope. It may seem a small difference in procedure to work from the new rope downwards, instead of making fresh calculations from base factors on each examination, but it is the sound, common-sense method. One must not forget that with a factor of safety of 4·5 as the minimum there is not so much room for errors of judgment without undue risk of incurring accidents as when the factor of safety is 6 as a minimum. In my opinion, nothing should be done to minimise the resident engineer's sense of responsibility with regard to ropes, and Mr. Price's suggestions seem to aim at "making things easy," which is to be deplored.

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

PAPER BY J. A. VAUGHAN (PAST PRESIDENT).

Discussion.

E. G. IZOD (Vice-President) contributed the following discussion to this paper:—

The writer feels that probably the author of the paper has been somewhat disappointed at the discussion which has taken place on his paper, but probably other members of the Institution have found the

same difficulty as the writer in criticising what is really a classic on the subject of wire ropes.

The discussion, so far, has rather centred round the figures the author puts forward in his paper, but the writer does not propose to deal so much with those points as with the question of the general principle which has been so ably put forward.

The author's proposition is to so amend the regulations that the minimum safety factor is now reduced from 6 to 4·5, with the added proviso that in no case shall the breaking load at any point therein become reduced to less than eight times the load carried at the lower end of the rope.

We have, therefore, two points to discuss:—

(1) The first, and, in the writer's opinion, the more important, namely, the 4.5 factor of safety, and

(2) that which the author calls the capacity factor or "y_1," which it is recommended should not be less than 8.

This capacity factor, or $y_1=8$, is introduced very skilfully to guard against the assumed detrimental effects of kinetic shocks. If, therefore, engineers are prepared to accept the author's finding on this subject of kinetic stresses on a rope, as set out in the paper before the S.A. Association of Engineers in March, 1904, then obviously that part of the proposed regulation making $y_1=8$ can be taken as satisfactory.

The writer has no knowledge whether it has been possible to prove by experiment the author's finding on critical depth, but as has been already stated, unless we are prepared to bring evidence to repudiate the finding of the formula for determining the critical depth or critical zone in which hoisting rope should work, then it seems only reasonable that the $y_1=8$ finding should be accepted without further demur.

One or two of the speakers have felt that if this portion of the regulation is adopted, it will create rather a hardship on some of the mines with shallow shafts, but in the writer's opinion the hardship is not nearly so great as at first appears.

On investigating the conditions governing the installation and taking off of ropes at some of the shallower shafts (for the sole purpose of giving an opinion on the $y_1=8$ system), it will be found that in few cases are ropes thrown out because a test has been made showing that the factor of safety is reduced below the present figure of 6 in the existing Mining Regulations. One case can be cited, namely, that of Modderfontein B. When $y_1=8$ was applied it seemed that the factor of safety suggested in the paper would create a hardship, but here a rope has run for four years and three months and was rejected with a factor of safety of $y_2=11·1$, y_1 in this case is far above the eight suggested for the new regulation, and if the writer had based his criticism of the capacity factor solely on calculation, it would have been of little value as compared with a criticism based on what is found to be satisfactory mine practice.

The writer has been able to make a further investigation into some of the shallower shafts, where at first sight it appeared that the $y_1=8$ factor would represent a hardship, and the accompanying table gives results of the investigation:—

TABLE.

Mine Shaft.	Depth of Feet.	Percentage Reduction that can be made on New Rope to bring down to $y_1=8$.	Percentage Reduction that can be made on New Rope to bring down to $y_2=6$ (Existing factor).
A	1,020	22·0%	29·0%
B	1,400	31·0%	31·0%
C	1,600	18·0%	18·0%
D	900	33·0%	40·0%
E	1,085	24·0%	30·0%
F	1,348	30·0%	30·0%
G	1.348	20·0%	27·0%

Here seven mine shafts were taken, and each was looked at from the following point of view:—

Assume a new rope is put on. Which will cause its rejection first? The new proposed regulation of $y_1 = 8$ or the existing regulation of $y_2 = 6$. It will be seen from the attached table in three cases there is practically no difference in the percentage reduction from the original strength given by the new regulation and the existing regulation. In the other four cases the difference is hardly worth discussion.

The writer is therefore prepared to support the author's proposition to amend that portion of the regulation covering the minimum factor of safety of eight, which, of course, takes no account of the weight of the rope.

The other portion of the recommendation is where it is suggested to reduce the safety factor (taking into account the weight of the rope and the load at the lower end of the rope), from 6 to 4.5. This is undoubtedly a step in the right direction, and will afford a great relief to some of our new deep level mining propositions.

The writer has several deep level propositions under consideration, and has had the privilege of discussing the question of deep level ropes with the author.

The reduction to the minimum of 4.5 as against 6 for a shaft 6,000 feet deep is a very material one, and while perhaps on some of the new deep shafts this factor might be even further reduced, yet the writer feels that all engineers will thank the author for having investigated the matter so far as to have suggested giving the substantial reduction now under discussion.

The writer is, however, still dissatisfied on this question of the safety factor of wire ropes used for winding for the following reasons. The position to-day is that under either the old regulation or the new regulation we take a formula or a calculation and settle the size of rope to be used by multiplying the total load by a figure based on a recommendation such as the author's, or a higher figure, if necessary, based on experience with the behaviour of ropes in a certain shaft and under certain conditions.

We sit down feeling quite satisfied that we have done the right thing, then at some period in the life of the rope there is possibly a jamb in the shaft or there is loose rope which suddenly tightens with a shock or many other conditions arising in the working life of a rope where, in the writer's opinion, all the calculations determined from the ultimate tensile strength of the rope are blown to bits.

The writer would therefore ask whether, in the author's opinion, there is not another test which will give us a further guide as to the best size and quality of steel or rope to be used.

The writer does not suggest that the tensile test of a rope on which deductions are based to-day is not satisfactory, but it is put forward as a definite opinion that another test is necessary if we are to obtain the best results from our winding ropes at a minimum of cost.

Consider for a minute the usual procedure in testing steel used for winding ropes or any other purpose. A test is made and a load determined at which the steel breaks down or ruptures, long after it has passed its elastic limit and is in a zone where its whole structure is destroyed and is of no further use. That the ultimate tensile strength is considered only a general guide in the selection of a rope is proved by the fact that a factor of safety of 6, 8, 10, or above this is used for other structural material apart from wire ropes; this factor has truly been described as a factor of ignorance, and it does appear to the writer that investigations might lead to other tests being adopted, which would assist very considerably in selecting a rope even more suitable than that selected solely on the ultimate strength as given by the testing machine.

It will perhaps be within the memory of the members of the Institution that when the British Engineering Standards Committee was considering the question of tests, they were approached by several engineers in England to have the elastic limit defined in the tests, so that the factor of safety could be based on the elastic limit and not on the ultimate tensile strength. The writer was one of those who gave opinion on the matter, and paid his tribute to what was, in his opinion, a wise move. Many other engineers of far wider experience than the writer also supported the proposal, but it was not adopted, and amongst the reasons given by the " Sectional Committee on Sections and Tests for materials used in the construction of ships and machinery " was one, that if the Standardisation Committee recommended the inclusion of the elastic limit in tests in order to use this figure for calculating the working stress, such pro-

cedure would dislocate the ordinary commercial testing carried out in the works test rooms.

As far as the writer is aware, there was not a single criticism against the advisability of the *principle* of basing working loads on a factor applied to the elastic limit, and the writer therefore contends that what might have been a real progress in testing and selection of materials was negatived principally on the poor argument that the new procedure would dislocate test room work.

The whole question is a very important one, but very few engineers, even though they admit the fact that other tests as well as the tensile tests should be used, are still unwilling to depart from old practice, and they continue their selection of materials for size and quality on the old system of testing.

The writer has one particular case in mind. About thirteen years ago there was a bad accident due to the bursting of a gun barrel under test. All the usual commercial tests were made on the material, and nothing whatever could be determined from such tests as to the reason why the gun barrel should have failed. At that time the writer was advocating the use of the Notched Bar Impact Test, and was asked to make tests on the broken gun barrel material and on the material of another gun barrel which had stood the proof tests with entire satisfaction. The material from the two barrels was subjected first to a complete tensile test, where the stretch modulus, the ultimate tensile, the elongation and contraction were determined with great accuracy. The two gun barrels gave exactly the same result, of course always within the limits of accuracy of the testing machine.

Two pieces were then cut from each gun barrel and submitted to the Notched Bar Impact Test. The test pieces from the gun barrel which had stood the proof test took 21 foot pounds to break them. The test pieces from the other barrel which had burst took 1.3 foot pounds to break them, *i.e.*, the material of the good barrel was able to withstand 16 times the impact stress of the material from the gun barrels which had failed. No other tests except the impact tests were able to determine the extraordinary difference in the material, and the writer simply puts this case forward as one small example of where, in his opinion, the tensile test fails.

On the Rand, engineers can congratulate themselves that the accidents due to broken winding ropes have been very few and far between, and it is quite possible that the adoption of the high factor of safety of 6 has been one of the reasons for the good behaviour of winding ropes.

The writer contends, however, that engineers should not be content to sit down and say thot they are satisfied, and he feels that he is quite within reason in saying that if another test by impact, or fatigue, or some such test as has been used by the author could be introduced, just as good results might have been obtained on Rand winding ropes with a factor of safety down to, say, even 4 or $3\frac{1}{2}$.

There is evidence that in the munition factories in England to-day, tests such as the impact test are being made in conjunction with other tests, in order to determine the most satisfactory class of material to be used, and in the writer's opinion this principle of using other tests beside the tensile test will grow very considerably in the future, and he would like the author in his reply to be good enough to give the Institution the benefit of his experience in testing wire ropes, as, if we are to progress on the lines suggested so clearly by the author, then there seems no reason why we should stop at the new proposed regulation, and if the author can throw more light on this subject then I am sure it will be welcomed by the members in the same way as it would be welcomed by the writer.

This little personal quarrel about the ultimate tensile test may perhaps be said to have led a little from the point under discussion in the author's paper, but the writer feels so strongly the importance of the subject that he makes no apology for interweaving the question with the proposals made in the paper.

The writer will throw out one further suggestion for the author's consideration, namely, that there may in the future be special cases of deep shafts where the conditions are such that the minimum safety factor proposed of 4.5 can be still further reduced, and if the Government Mining Department could treat some of these cases as special ones and consider each on its

merits, further relief would be given than under the present proposals.

The minimum factor of 4.5 seems such a hard and fast figure to apply when it is known that the conditions of winding, such as speed, condition of shaft, etc., vary so considerably, and it seems to the writer that these conditions might be taken account of in determining the margin of strength to be used in a winding rope, and if the author of the paper would take this point into consideration, all engineers will, it is felt, be more than grateful.

In concluding the few remarks, the writer would like to add his testimony to that of other members in thanking Mr. Vaughan for such an excellent contribution, and whichever way the author's paper is looked at, and whatever the criticisms may be, there is no doubt that, as before, the Rand can "report progress."

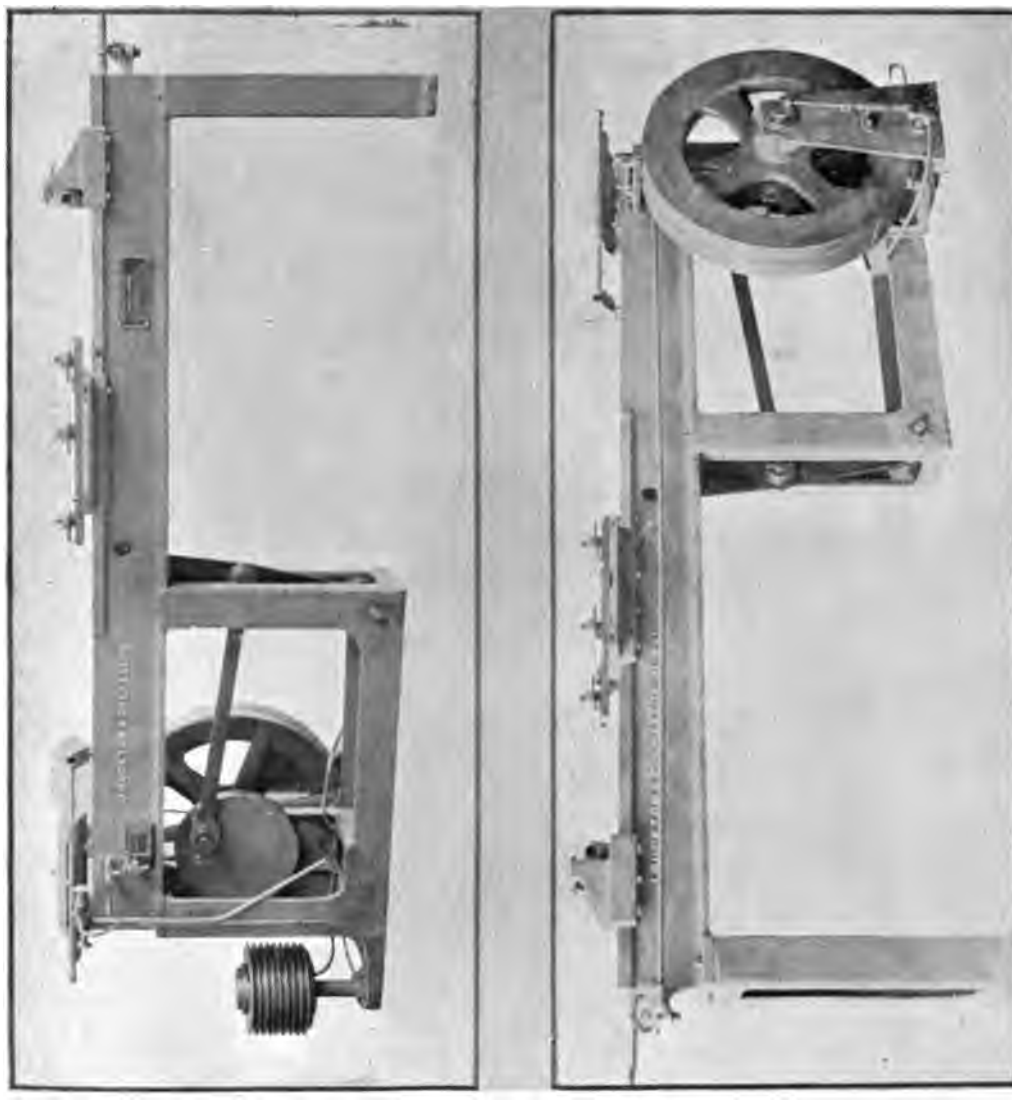
E. J. LASCHINGER (Past President) said he had spent some time in reading Mr. Vaughan's paper, but the ideas he had evolved in his own mind up to the present were not quite definite, and before he made any further contribution to the discussion he would like to ask Mr. Vaughan a few questions, so that he could embody the results of his experience with regard to the testing of wire ropes in his conclusions. The first question he would like to ask Mr. Vaughan was as to what was the amount of wear, as actually determined by the loss of weight in a certain length of rope, between the time that a new rope was put into use and the time it was discarded. An average figure on that point would be instructive. The next question was with regard to an average figure dealing with the diminution of the tensile strength. What did Mr. Vaughan think was a fair average figure to assume for diminution in the tensile strength of the rope from the time that it was first put on as new and the time when it was discarded as unfit? The third question was with regard to the deterioration in the quality of the rope, not as affecting the tensile strength, but as affecting its flexibility or brittleness—that is, its ability to withstand the shocks and kinetic stresses or the effect of flexure round the pulleys.

He quite agreed with Mr. Izod that tensile strength was not the only factor, although it might be an important factor. Various materials giving practically the same tensile strength very greatly depended, in their suitability for withstanding rupture when it came to shocks, upon the factor which might be known as the factor of brittleness or flexibility, or some other name of that kind. He would be very grateful if he could get some rough idea from Mr. Vaughan upon these few questions before he contributed further to the discussion on this most valuable paper which had been presented by Mr. Vaughan to this Institution.

J. A. VAUGHAN (Past President) said he was not prepared that night to answer all the very leading questions put to him by Mr. Laschinger. He could say, however, that all the information, as it existed in the tabulations at the Laboratory, was at his service. A great deal of information had already been given to the Institution. For instance, the question as to whether a winding rope had lost the strength which one would assume it had lost by the maximum reduction at certain points in the strength of its wires as separately tested, had already been considered and tables had been given by Mr. Epton (*Journal, Transl. Inst. Mech. Eng.*, Vol. V., No. 11, p. 244) showing that, although in some instances there was some mutual support between the separate wires of the rope, it was always better to conclude that the rope was not stronger than the aggregate of its wires.

With regard to the subject of brittleness and the second of Mr. Laschinger's questions, he thought the best thing the latter could do was to go to the Laboratory and search into the records that they had there; this would be much more satisfactory both for Mr. Laschinger and himself.

E. J. LASCHINGER said that all he wanted was a general idea. For him to go through the thousands of tests that had been conducted by the Government Mining Engineer's Department was impossible. Mr. Vaughan based his conclusions upon a general factor of tensile for strength, and therefore all he (the speaker) wanted was to get information as to the general influence of those factors upon the strength of a winding rope, a general reply to the question as to the amount to be deducted from the legitimate wear of outside or inside wires as a factor. Then there was a general factor for the reduction of tensile strength between new ropes and discarded ropes.



THE VAUGHAN-EPTON FATIGUE-TESTING MACHINE.

The other item was with regard to the ability of new rope, and as to the behaviour of old rope after the flexure test, for which, he understood, there was a machine. Mr. Vaughan proposed new regulations based upon a general average.

J. A. VAUGHAN said that so far as tensile strength was concerned, judging from the outside wear merely, if the wires of a rope had worn one-third, generally, its weight would be reduced by a much smaller fraction. At the same time if the rope had corroded externally and internally the loss of weight might be even greater than the loss of tensile strength, and, as far as ductility was concerned, the wires might not be worth anything. Mr. Laschinger, as a practical engineer, knew just the positions in the rope where these maximum efforts generally occurred, and would not suggest that these points of maximum wear and deterioration as to brittleness would be at the upper end; they were met with near the lower end of a rope, except in the case of the rope in a compound shaft after being changed end for end. They had at the Laboratory tabulations which were at Mr. Laschinger's service, also the percentages between weights of old and new ropes; but he was not prepared that night to answer those questions any further than he had done at present unless Mr. Laschinger asked them more explicitly.

W. MARTIN EPTON (Past President) said he was particularly interested in some of the remarks Mr. Izod had made that night; it might be of interest to members to hear of other kinds of testing than the ordinary tensile test. There was at present a machine in the Mines Department Laboratory known as the Vaughan-Epton testing machine, designed for the particular purpose under consideration. This machine was the outcome of attempts to decide by the ordinary test methods the relative merits of different priced wires available for rope manufacture. There was a wide difference in price, one wire being as much as double the price of another of the same size.

The old test methods referred to were those involving successive bendings over a fixed radius, the torsion and the tension tests. These tests were also in some cases super-imposed, but no differentiation resulted. The stress strain diagrams in the tension test, microscopic examination of fractures, and the standard chemical test all failed to show the superiority of the more expensive wires, yet the latter were known by rope manufacturers to give better results in practice. The idea then occurred to apply the ordinary tests simultaneously, and the Vaughan-Epton machine was the result. A regulated tension can be applied of any amount, and this load remains the same in spite of all stretch in the wire. The reciprocation of the table carrying the sheaves at the same time applies the alternating bending stresses to the wire under test. A diagram is taken combining strain (*i.e.*, stretch) with the number of reversals, and may be used with certainty to identify the grade of wire. This has been proved by experiments stretching over several years.

It would be evident from the accompanying photograph of the machine in question how the test was applied. The autographic recorder was not, however, included in the photo.

J. A. VAUGHAN said that this particular machine was of no use in dealing with the separate wires of a worn rope; it would simply tell what one was buying or what one proposed to buy in the case of a new rope. As a rule, too, one could not judge much from the tests made of worn wires bent across the ordinary quarter-inch diameter radius. From the fatigue test results of an unworn wire they could tell what it was likely to do and how it would stand against adverse conditions in working.

E. J. LASCHINGER said that from Mr. Vaughan's remarks it seemed to him that Mr. Vaughan was basing his proposed new regulation upon the fact that the results of the testing machine did not give a true indication of what the value of the rope was. He thought that that condition of affairs might be rectified. Wires might be tested by simply grinding off the abraded surface so as to make the surface parallel, and then test the wire so as to see what the quality of the material was. They wanted to test the quality of the material. The amount of deterioration that had taken place in a wire was not indicated by the tensile testing machine, because they knew very well that wire which was very resilient and which was able to take up the shocks was the kind of wire they expected to buy when they got a new wire rope; but when the old rope was worked dead they knew that the resiliency of the wire had decreased very materi-

ally. He thought that in the accidents which happened to-day—and there were not many—the chief cause of the trouble had been that the rope had deteriorated to such an extent that it had not been able to withstand kinetic shocks. He did not see why the same machine which was invented and devised by Mr. Vaughan and Mr. Epton should not be able to give them an indication of the amount of deterioration in the wire between the time when the rope was first put on and at any time it was tested afterwards when it was discarded. The state of deterioration of a wire between the time it was tested new and when it was tested old was the important point.

J. A. VAUGHAN said he could answer this point straight away. The wires that gave them the greatest worry were the wires at the lower end of the rope, which had been compelled to accelerate and retard the pulleys of the bend or any diverting pulley. If they adopted Mr. Laschinger's suggestion and ground off the face of them they would have a very good wire. This was very well known, and he thought it had been stated publicly many times that, owing to the rough use those wires had been subjected to, they had been case-hardened on the surface, and if they examined them under the microscope they could see the minute cracks that had developed. On several occasions they had tested wires of, say, ·1 inch diameter, only about one-third worn, and they could break them in their fingers as they would a match. If they ground off the top of the surface they got a wire with what might be described as having the resilience still in it; it was still good.

E. G. IZOD said he would like to make one point in his discussion quite clear. His suggestion to make other tests besides the tensile test was meant to be applied to ropes before they were put into use.

J. A. VAUGHAN said that a fatigue testing machine was quite a good idea. The diagram from the fatigue testing machine identified the wire just as the finger prints identified the criminal.

PERCY CAZALET (Member) said he wished to add his meed of thanks to Mr. Vaughan not only for the enormous amount of trouble and work entailed in the compilation of his paper, but also for the fact that it proved that the mining industry had a public servant with the moral courage of his convictions to come forward and tell members that mining regulations could be modified. They were extremely fortunate in the mining industry in having men, to mention only two, like Mr. Kotzé and Mr. Vaughan in superior positions under Government. If they had not such men as these they could conceive of cases of, say, very deep mines being unduly handicapped or prevented from working at all, merely through the rigidity of certain of the regulations.

T. M. SMITH (Member) thought the Government might be induced to spend money on a machine sufficiently large to test the rope instead of the wire.

J. A. VAUGHAN said that before the War started, the Institution of Mechanical Engineers in London had formed a committee with which he was in correspondence, and they, he believed, proposed to do actually the same thing that the last speaker proposed. If they could only get the War finished this might get it done.

J. W. KIRKLAND (Member) asked whether it was not a fact that almost all breaks that had occurred in winding ropes had occurred very near the skip, and whether it was not possible that the fact that they had had so few disasters in winding ropes had not been due to ropes being cut off for testing and so shortened from time to time. Accidents had been avoided because ropes had been tested near the load they were lifting.

T. M. SMITH said that, in incline shafts more notably, the smallest diameter of rope was usually at a point 100 to 200 feet from the skip, and, generally speaking, this part could only be tested after the rope was discarded, which he believed was sometimes done and useful data procured in consequence.

J. A. VAUGHAN said that if the regulation regarding re-capping and testing did not exist there they would not be able to find the flaw. On about half-a-dozen occasions in a year they had been able in the laboratory to point out to an engineer matters which even the rigger had not been able to see. They had very good riggers, but he fancied that sometimes deterioration was not actually in evidence. There was some inherent defect in the rope, which became evident in testing. He thoroughly agreed with Mr. Kirkland when he said that the great point was to keep shifting the positions of greatest wear, and, with that purpose in view, to provide as much spare rope

on the inner end as one could. The danger of turning a rope end to end on a compound shaft was the chief danger they had on these mining fields.

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

PAPER BY J. A. VAUGHAN (PAST PRESIDENT).

Continued Discussion.

J. R. COWELL (Member) contributed the following to the discussion on this paper, and exhibited photographs and specimens in connection therewith:—

With reference to the discussion on Mr. Vaughan's excellent paper on wire ropes, there is one factor which has to be specially considered in connection with the determination of safe working limits, and that is the difference in the construction of and quality of material in ropes used for vertical hoisting and those used on incline or compound shafts.

For vertical hoisting the quality of the wire should be of the very best as regards torsion and ductility tests. The writer, however, questions very much if the same can be said to apply to ropes used for incline or compound shafts. For these latter ropes fatigue, impact, chemical or torsion tests do not necessarily show the suitability of wires for such ropes, although they are essential for ropes for vertical shafts. In the writer's experience it has been repeatedly shown that the highest grade of steel, when put into ropes used in incline and compound shafts, has proved an absolute failure, whilst much lower and cheaper grades of wire of equal ultimate breaking stress have been quite successful. A standard specification of wires for ropes for all purposes would be therefore worse than useless.

In support of the writer's contentions I submit herewith some interesting remarks in connection with some ropes supplied by the firm with which I am connected, and which also confirm some of the remarks made by Mr. Vaughan at the last meeting in reply to Mr. Laschinger.

Some two years ago we sent Home samples cut off two worn ropes which had been supplied to us by Messrs. Wilkins' Wire Rope Co., Ltd., for investigation by that firm, with the idea of trying to obtain the most suitable material.

The material of which the ropes were made was of the best Swedish charcoal material. They were about 1·45 in. diameter, and constructed respectively of six strands of 16 wires each and six strands of 17 wires each. These ropes had been taken off an incline shaft on account of broken wires, but it had been expected that the ropes would have eventually been taken off from reduction of area rather than from broken wires, owing to the high grade of material employed.

After the ropes had been sent Home to the manufacturers for examination, they were sent on by them to Mr. J. E. Stead, the eminent metallurgist, for investigation, and he reported as follows:—

"In conformity with your instructions, I have most carefully examined the two pieces of wire rope, 4 ft. 2 in. in length, received from you.

The respective ropes were labelled:

A. 200 feet from skip end.

B. 340 feet from skip end.

Description and Particulars.

A. Contains six strands with hemp core. Each strand consisted of:

10 outer wires 2·82 m.m. in diameter.
6 inner wires, 2·2 m.m. in diameter.
1 soft core wire.

The lubrication internally was good. There were only slight traces of rust.

There were 13 broken wires, four of which fell away when opening out the strands.

The crown wires were fairly evenly worn.

Distance between worn crowns in outer wires from each other, 6·7 inches.

All the crowns of the outer wires were too hard to scratch with a hard steel point, but after grinding off one-hundredth of an inch the metal below could be easily scratched.

On slight bending the crown wires cracked and fracture passed through the wires.

B. Rope contained six strands with hemp core. Each strand consisted of:

9 outer wires, 3·03 m.m. diameter.
6 inner wires, 2·00 m.m. diameter.
1 soft wire core.

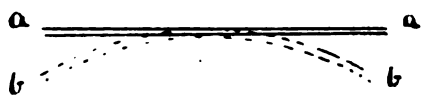
The lubrication was good, and there was no sign of rusting.

There were 13 broken wires, three of which fell away on opening out the strands.

Distance between the worn crowns in outer wires from each other, 6·6 inches.

All the crowns of the worn wires were exceedingly hard, and could not be scratched with a hard steel point, whilst the undersides could be scratched.

On slight bending from aa to bb,



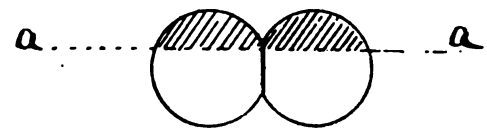
the surface layers broke and many hair-like cracks appeared. On further bending complete fracture invariably resulted.

Analyses were made of each rope, excluding the soft core in each case.

The analyses indicate that the material itself is quite perfect.

The question as to what caused the excessively hard skin was answered by the results obtained on microscopic examination.

Two of the crown worn surfaces of one of the outer wires of each rope were placed face to face and secured in that position. They were ground down to the points a a, as shown in the sketch below, and the surfaces were polished, etched and photographed:—

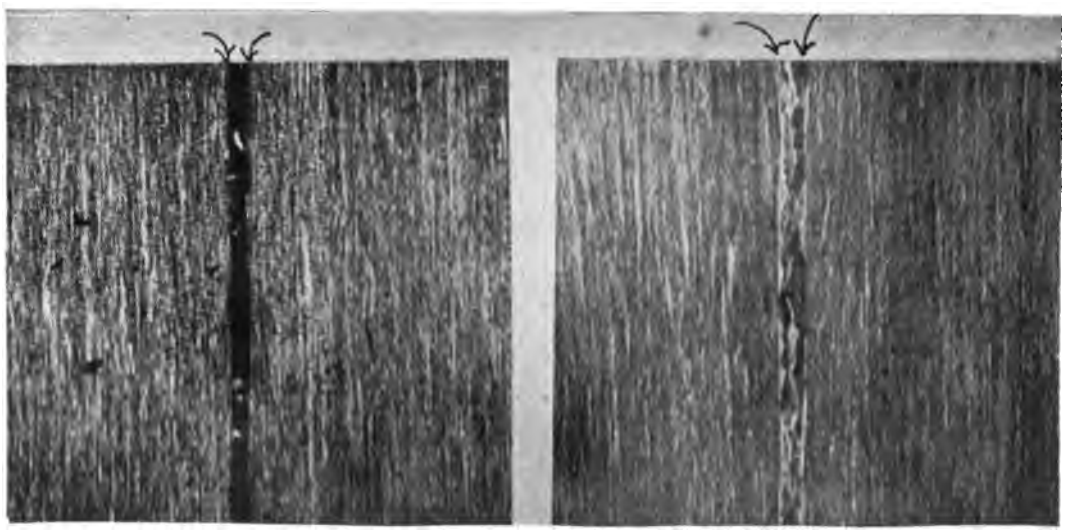


The photographs will be seen on the accompanying cards.

Rope B.

Crown worn wire, initially free from cracks, after slight bending—a process which causes the hard skin to break into a multiude of hair-like cracks. Magnified 50 diam.

It will be noticed that there is no evidence of any white layers of steel hardened by heating and quenching, which there would have been had excessive friction, and consequent heating followed by rapid cooling, been the cause of the hardening. I am forced to conclude, therefore, that the hardening is the result of excessive pres-



Rope A.
Section through two crowns in juxtaposition.
Etched with acid. × 50.

Rope B.
Section through two crowns in juxtaposition.
Etched with acid. × 50.

sure and friction, which has had the same effect as excessive overdrawing, but in a much more pronounced degree. This conclusion is confirmed by the amount of steel in the form of exceedingly thin plates which were separated from the lubricant in a single strand. Much of it was lost, but at least one gram was actually weighed, a portion of which is returned to you fixed on to the surface of one of the cards which accompany this report. This material was present in each rope, but more in "B" than in "A."

During use the surfaces of the rope have evidently been pressed too strongly against the pulleys; the steel has been forced to flow along the surfaces, and eventually has escaped from the sides of the crowns, and been caught in the grease between the wires.

The steel itself is of excellent quality, and has been drawn to its maximum strength, but, as will be shown below, not beyond that point.

As the wires were more or less crushed on the undersides below the crowns, it was impossible to get satisfactory results on testing for torsion and tenacity, and no attempt was made in that direction. Other tests, however, were made which showed that the steel was excellent.

A series of pieces cut from between the worn crowns was filed down on one side only, so as to expose a series of surfaces at progressive distances from the outside. They were then bent over so as to put the exposed layers in tension. It was not until the steel in the central axis was exposed and bent close to 180° that any sign of weakness appeared. All the best wires experimented upon, adhering to cards, are sent herewith.

List of Exhibits.

A.—*Sheet 1.*

		Diameter. mm.
1.	Unworn wire	2·82
2.	Unworn wire, ground on one side ...	2·60
3.	Unworn wire, ground on one side ...	2·2
4.	Unworn wire, ground on one side ...	1·6
5.	Unworn wire, ground on one side ...	1·4
6.	Unworn wire, ground on one side ...	1·2
7.	Crown of wire, slightly bent	2·50
8.	Crown of wire more bent	2·45
9.	Crown of wire broken on bending...	2·50
10.	Crown of wire ground off...	2·40
11.	Crown of wire ground off...	2·30
12.	Crown of wire ground off...	2·00
13.	Crown of wire ground off...	1·50
14.	Crown of wire ground off...	1·20
15.	Wire hammered on its side.	

B.—*Sheet 2.*

		Diameter. mm.
1.	Unworn wire	3·03
2.	Unworn wire, ground on one side ...	3·00
3.	Unworn wire, ground on one side ...	2·80
4.	Unworn wire, ground on one side ...	2·70
5.	Unworn wire, ground on one side ...	1·90
6.	Unworn wire, ground on one side ...	1·90
7.	Unworn wire, ground on one side ...	1·60
8.	Unworn wire, ground on one side ...	1·50
9.	Crown wire bent	2·70
10.	Crown wire bent	2·70
11.	Crown wire bent	2·75
12.	Crown wire bent and broken	2·70
13.	Surface of sound portion of wire filed off and wire bent.	
14.	Steel dust extracted from lubricant of rope.	

Wires Nos. 7 and 8 on Card No. 2 show fracture has occurred in the central part of the wire, suggesting a tendency to break up with a cup and cone fracture. It was, however, not until the wires were bent almost close that these fractures appeared. I am satisfied, therefore, that as nothing like this extreme bending occurs when the rope is in use, this localised slight tendency to break would not tend to reduce the life of the rope.

It is my opinion that had the wire not been drawn to give maximum strength, this slight local weakness would not have presented itself, but this is open to proof.

The other bent wires show the material to be excellent.

In order to find out whether the steel below the hardened and tortured crowns had suffered, a series of the worn crown wires were ground off to varying and increasing extent. The wires were then bent, and, as will be seen, the brittleness is entirely confined to the skin or surface layers, for after these were removed the parts below were found to be quite equal to the corresponding parts of the unworn wires.

In all my long experience I have never before met with a case such as this, proving that excessive friction, combined with excessive pressure, has produced such extreme surface hardness.

Few initial cracks could be detected in the worn crowns, but on very slight bending, the perfectly sound crowns in every case cracked at once. A photograph was taken of one of these after slightly bending.

Tests were made to determine whether or not the wires had been overdrawn.

Experience has proved that if wire has been reduced beyond the safe limit, on hammering the wire on its sides it readily breaks up into fibre. All well-drawn wire does break up when treated in that way, but it takes a good deal of hammering to produce longitudinal fractures.

On submitting the wires of each of these ropes to this test it took much hammering to effect splitting. I am satisfied, therefore, that the wire-drawer has not exceeded the safe limit, and the wires are not overdrawn. Overdrawn wire usually splits longitudinally along the worn crowns when the ropes during service have been subjected to excessive pressure, but, in the ropes in question, nothing of the kind is in evidence.

It is to be noted that the ropes failed at distances of 200 and 340 feet from the skip end, presumably at points where the excessive crushing was at a maximum during use.

It would have been interesting if we could have compared the portions sent with parts cut from nearer the drum end when the tensional stresses approached nearer to the maximum."

From this report it is quite evident that the breaking of the wires was entirely due to the excessive friction and pressure on the wires, caused either by lashing or contact with rollers. It has been found, in my experience that ropes of a lower quality of steel, of same ultimate tensile strength, have given better results.

To sum up the drift of my remarks, my contention is that the highest-grade or purest steel rope generally is the best for vertical hoisting, whilst it is not the best for compound or incline shafts. By the highest grade, please understand I do not mean highest grade in carbon or ultimate breaking stress per square inch, as high-grade ropes may be from crucible steel 80 tons per square inch B.S. up to high carbon plough of 150 tons per square inch B.S.

E. J. Laschinger (Past President) contributed the following discussion on the paper. He said:

The thanks of the Institution are due to the author for his paper, and he especially deserves the thanks of the mining world for putting into the form of a definite proposition a matter which has been exercising the minds of mining engineers for a good few years past. It has been long recognised by those who have made a study of and investigated deep-level winding problems that the safety factor of six at the upper end of a loaded rope could and should be reduced in deep winding with advantage to the mining industry without sacrificing safety in operation. Indeed, for very deep winding, the present Mines and Works Act of the Union of South Africa imposes prohibitive restrictions in certain cases.

Mr. Vaughan's proposition is to fix two limits for the factor of safety and to embody these conditions in the mining regulations, and it is therefore very important for engineers here thoroughly to discuss his proposition and to criticise the effect of such conditions on the practical operations of the mines, existing or contemplated.

The proposed conditions are:

(1) To fix the higher static factor of safety of the rope where attached to the load (designated as " y_1 ") at *eight* as a minimum; and

(2) to fix the lower static factor of safety at the upper end of the rope when the load is at its maximum depth (designated as " y_2 ") at *four and a half* as a minimum.

If either factor of safety becomes less than the above figures the rope must be discarded.

This of course means that when a new rope is put on the initial factors of safety must be much higher to allow for:

(1) Wear resulting in reduction of cross-sectional area of steel.

(2) Reduction in tensile strength of steel in wires.

The proposition does not take any account of the ability of the material of the rope to withstand shock or flexure, which might be termed a factor of brittleness or factor of suitability for the nature of the service required. This point was referred to by Mr. Izod at the last meeting in his contribution to the discussion.

It has also been pointed out that the proposed new regulation might bear hardly upon shallow mines.

Before entering upon a discussion of the points above mentioned the writer will first enter upon an investigation of the static

factors of safety and their relations to one another and to depth.

It is well known that ruptures of the rope (outside of causes external to the rope itself) always, or nearly always, occur at the lower end, where the static factor of safety is greatest. The principal cause of this is also well known, being stresses due to kinetic shocks, which at this end are cumulative in effect up to a certain depth of winding, and may mount up to an indefinite magnitude, depending upon conditions. This part of the rope is also most subjected to wear. Another reason for this is the deterioration of the rope due to the occlusion of hydrogen gas, resulting from the action of acid water or acid gases on the steel, which is most prolonged at the lower end of the rope; further, also it must be remembered that the lower end is constantly under strain and gets no relief, whereas the upper end of the rope is partly and, in case of winding on the drum in layers, completely relieved of tensile stress owing to the contraction of the drum shell as more coils or layers of rope are wound on. In certain cases even the upper end of ropes have been damaged when being covered with numerous layers of rope, because the contraction of the drum shell was so great that the first layer of rope, especially if not unwound for some time, was put into compression, and the strands opened out and wires were buckled.

It is therefore evident that a minimum static factor of safety (y_1) for the lower end of the rope is a reasonable condition to impose in a regulation if fixed at a reasonable figure.

With regard to the static factor of safety (y_2) at the upper end of the rope, it is undoubted that such must also be fixed by regulation, as it gives a definite and handy measure of assessing the value and safety of a rope. It will be conceded, however, that this minimum factor of safety should be governed by some relation to the depth of winding with a certain irreducible minimum as a starting point. It is here that the chief problem lies. The fixing of y_2 is in the nature of the standard set in a school examination in order to test the educational qualifications of the pupils; it is a handy method, not a full criterion, but must stand until something better is evolved and can be adopted.

In approaching this problem of fixing the minimum factors of safety for the upper and lower ends of the rope the writer started with the investigation on this basis:

Allow a factor of safety at critical depth $y_{2c} = 6$, and for greater depth the same sized rope could be used until at ultimate depth $y_{2u} = 4{\cdot}5$.

By critical depth is meant the depth of shaft such that the length of rope will, according to Mr. Vaughan's definition and calculations, not allow kinetic shocks to accumulate. This length, as shown by the author, is such that the weight of the suspended rope is 0·35 of the weight at the end of the rope.

In working out the above problem the algebraic results give us limits which are applicable for any chosen factors.

The writer has adopted Mr. Vaughan's nomenclature and symbols, except that the letter " L " represents the length of rope instead of " x," because the symbol "x" is rather confusing in arithmetical work.

The symbols, therefore, are:—

a = load on end of rope in lbs.
L = length of rope in feet.
S = cross-section area of steel in rope — square inches.
t = tensile strength of steel (short tons per square inch).
y_1 = factor of safety lower end.
y_2 = factor of safety upon upper end.

Taking the author's assumption that the weight of rope = 4SL, which is substantially correct, and that the laid-up strength of a rope is 90 per cent. of that of the aggregate wires, we have:

For winding in vertical shafts—

$$y_2 = \frac{1800St}{a+4SL} \qquad (1)$$

$$y_1 = \frac{1800St}{a} \qquad (2)$$

Let L_c = critical depth,

and y_{2c} = upper factor of safety at critical depth.

At critical depth therefore

$$4SL_c = 0{\cdot}35a \qquad (3$$

or $$a = \frac{4SL_c}{0{\cdot}35} \qquad (4)$$

From equations (1), (2) and (4) we have

$$y_1 = 1{\cdot}35y_{2c} \quad (5)$$

and $$y_{2c} = 0{\cdot}74y_1 \quad (6)$$

$$L_c = \frac{116{\cdot}5t}{y_{2c}} \quad (7)$$

or $$L_c = \frac{157{\cdot}5t}{y_1} \quad (8)$$

It will thus be noticed that by choosing one of the factors of safety the magnitude of the other at critical depth is fixed, and by choosing either of the factors of safety and the tensile strength quality of the steel the critical depth is also determined.

TABLE A.

The factor of safety at critical depth being chosen and strength of steel—gives factor of safety at lower end and critical depth.

$y_1 = 1{\cdot}35y_{2c}$ $\quad L_c = \frac{116{\cdot}5t}{y_{2c}}$

Factors of Safety.		Critical Depth. Feet.	
		Strength of Steel.	
y_{2c}	y_1.	t=100.	t=120.
3	4·05	3,890	4,670
3·5	4·73	3,330	4,000
4	5·40	2,920	3,500
4·5	6·08	2,590	3,110
5	6·75	2,330	2,800
5·5	7·43	2,220	2,540
6	8·10	1,940	2,330
6·5	8·78	1,790	2,150
7	9·45	1,665	2,000
7·5	10·12	1,550	1,865
8	10·80	1,457	1,750
8·5	11·48	1,370	1,645
9	12·15	1,293	1,570
9·5	12·83	1,225	1,475
10	13·50	1,165	1,400

TABLE B.

The factor of safety at lower end of rope being chosen and strength of steel—gives factor of safety at upper end at actual depth and critical depth.

$y_{2c} = 0{\cdot}74y_1$ $\quad L_c = \frac{157{\cdot}5t}{y_1}$

Factors of Safety.		Critical Depth. Feet.	
		Strength of Steel.	
y_1.	y_{2c}.	t=100.	t=120.
4	2·96	3,940	4,730
4·5	3·33	3,500	4,200
5	3·70	3,150	3,780
5·5	4·08	2,867	3,440
6	4·44	2,630	3,150
6·5	4·82	2,420	2,910
7	5·19	2,250	2,700
7·5	5·55	2,100	2,520
8	5·93	1,970	2,365
9	6·66	1,750	2,100
10	7·40	1,575	1,890
11	8·14	1,430	1,720
12	8·89	1,310	1,575
13	9·63	1,210	1,455
14	10·36	1,125	1,350

The accompanying Tables A and B give figures for different assumed factors of safety for 100 and 120 ton steel. It will thus be seen that for $y_1 = 8$, $y_{2c} = 5{\cdot}93$, or for $y_{2c} = 6$, $y_1 = 8{\cdot}10$, which means that practically Mr. Vaughan's proposition of making $y_1 = 8$, the factor of safety at the critical depth would be 6, which is in accordance with the existing regulations. The critical depth for an ordinary class of high-quality wire would be about 2,000 feet.

Let L_u be the ultimate depth and y_{2u} be the minimum factor of safety, then from the former equations,

$$\frac{y_{2u}}{y_{2c}} = \frac{3{\cdot}68L_c}{2{\cdot}86L_c + L_u} \quad (9)$$

$$\text{and } L_u = L_c\left(3{\cdot}86\frac{y_{2c}}{y_{2u}} - 2{\cdot}86\right) \quad (10)$$

or with Mr. Vaughan's proposition of fixing y_1 and y_{2u}

$$L_u = 2{\cdot}86L_c\left(\frac{y_1}{y_{2u}} - 1\right) \quad (11)$$

By making the factor of safety y_{2c}, *i.e.*, factor at upper end when load is at critical depth equal to *six*, and the permissible factor when load is at ultimate depth equal to *four and one-half*, then equation (10) the

ultimate depth at which the rope could be used would be:

For 100-ton steel, 4,450 feet.

For 120-ton steel, 5,340 feet.

Taking Mr. Vaughan's proposition of making $y_1=8$ and $y_2=4{\cdot}5$ as the limit, then the ultimate depth to which the rope could be used would be:

For 100-ton steel, 4,375 feet,

For 120-ton steel, 5,250 feet,

$$\text{since } L_u=450t\left(\frac{1}{y_{2u}}-\frac{1}{y_1}\right) \qquad (12)$$

which equation is identical in result with equation (11).

Mr. Vaughan's proposition is therefore a very ingenious method of assuming that any rope complying with his condition has a minimum factor of safety of not less than six up to the critical depth, and thereafter the permissible factor is allowed to be reduced to a minimum of four and one-half. The proposition is therefore an eminently rational one, and a distinct step in the right direction which should meet with the approval of the authorities.

With regard to the other phase of the proposal—that this new regulation would impose a hardship in the working of shallow shafts—it is evident that the new regulation is not so favourable as the existing one. The new regulation, by fixing $y_1=8$, means that for any shaft hauling from a depth less than the critical, the factor of safety at the upper end is always greater than *six*. As a measure of safety it is therefore on the conservative side. The writer, after making certain investigation on ropes in actual use for shafts of less than the critical depth, can show that in general no great hardship or abnormal expenditure would be incurred by adopting the standard of $y_1=8$ as a minimum; nevertheless, considerable expense might be saved in ropes hauling very heavy loads from depths less than the critical if the old factor of $y_2=6$ were retained in such cases. A very substantial saving would also be effected in the cost of the winding engine enquipment because of the smaller size of drum required, since the size of drum which will give equal flexure or bending strain is in direct ratio to the diameter of the rope wound on. It is also well known that the size and capacity of steam cylinders or electric motors for winders is governed by the starting torque, and not by horse-power capacity, and this starting load is in direct proportion to the diameter of the drums, and therefore governed by the diameter of the rope used.

On the whole, therefore, the writer is of opinion that Mr. Vaughan's proposal is a sound and safe one, and would prepare the way for further improvement in the regulations with regard to conditions of winding.

If the conditions of the proposed regulation could be made so that the present minimum factor of $y_2=6$ could be retained for anything up to the critical depth (or more properly speaking, the critical length of rope), and the condition $y_2=4{\cdot}5$ and $y_1=8$ for greater depths or lengths of rope, then no hardship or unnecessary expense would be incurred over present conditions, and for great depths substantial relief would be afforded without sacrificing safety. The critical depth would, of course, have to be clearly defined. It may also be borne in mind that such a regulation would not involve a "stepped" factor of safety, since the two conditions merge into one another at the critical depth.

It will have been noticed that the writer has used the terms "critical depth" and "critical length." In vertical haulage the two terms are synonymous, but as the cumulative effect of kinetic shock is dependent upon the mass at the end of the rope and mass of rope, therefore, in incline haulage critical depth and critical length of rope are not synonymous.

The cumulative effect of kinetic shock is well illustrated in the breakage of couplings of railway carriages. The factor of safety on direct tensile stress and tensile strength of the couplings is enormously high, but, owing to the short length of the couplings, the cumulative effect of kinetic shock when heavy masses have to be moved is incalculable, and must be very high compared with the direct traction effort.

Without having the statistics to prove his point in a conclusive manner, the writer is of opinion that the greater number of rope failures in inclined shafts as compared with vertical shafts is due largely to the fact that, given equal factors of safety (y_2) at the upper end of the rope, the mass at the end of the rope compared with the mass of the rope is much greater for incline than for vertical winding, and therefore kinetic

shocks are much more severe. The writer is, of course, aware that ropes used in incline shafts suffer from wear due to dragging and flexure over small rollers, etc., which conditions do not apply to vertical shafts. In incline shafts there are also accidents due to derailment which occur more frequently than with skips or cages leaving the guides in vertical shafts.

In an inclined shaft the critical length of rope is greater than in a vertical shaft, but theoretically with the same safety factor the critical vertical depth is the same in both cases. For instance, on an incline of 30 degrees the length of rope which will weigh the same as that on a vertical haul on the same load will be twice as great with the same factors of safety in each case.

A simple mathematical investigation will show that the relation of the factor of safety at the lower end (y_1) of a rope used in an inclined shaft to the factor at the upper end (y_2) is the same as for a rope used in a vertical shaft, as we have

$$y_{2i} = \frac{1800St}{(a+4SL \sin i} \quad (13), \text{ compare} \quad (1)$$

$$y_{1i} = \frac{1800St}{a \sin i} \quad (14), \text{ compare} \quad (2)$$

By assuming Mr. Vaughan's proved proposition that the effect of kinetic shocks is not cumulative after the mass of rope is greater than 0·35 of the mass at the end of the rope, *i.e.*,

$$4SL_{ci} = 0{\cdot}35a$$

where L_{ci} is the length of rope in an incline at critical depth it follows that

$$L_{ci} = \frac{116{\cdot}5t}{y_{2c} \sin i} \quad (15), \text{ compare} \quad (7)$$

$$\text{or } L_{ci} = \frac{157{\cdot}5t}{y_1 \sin i} \quad (16), \text{ compare} \quad (8)$$

It would thus appear that a higher factor of safety is required for ropes in incline haulage than for vertical haulage except with very great lengths of rope, but the two cases are alike (theoretically) at equal vertical depths. A modifying influence, however, is introduced by the fact that kinetic shocks would in practice not be likely to be so severe in inclined as in vertical shafts, more especially as incline winding is not so rapid as vertical winding.

With regard to the effect of kinetic shock, a good deal of stress must be laid on the factors (1) in the appendix to Mr. Vaughan's paper—*i.e.*, that "a wave transmitted along a wire rope will probably suffer considerable diminution in intensity due to friction between the strands and wires, etc.," and (2) that the length of the spiral being greater than that of the rope, the rate of travel will be lessened. This means that the critical depth will be less than that calculated, and also that ropes are really safer than we assume, more especially at great depths.

In all the above calculations and examples it is the worn rope just before it is to be discarded that is assumed. In choosing a new rope, therefore, due allowance must be made for the probable wear and any reduction in tensile strength with use and age, and it is in assessing such factors that the experience and judgment of the engineer responsible is of the utmost value. It was to elicit information from Mr. Vaughan, in his wide experience of the testing of ropes that the writer asked certain questions at the last meeting of the Institution, and still hopes that the author will give some information on these points in his reply.

The writer can quite understand the author's diffidence in proposing any further amendments or additions to the mining regulations, such as laying down definite rules or tests regarding the ability of the wires of a rope having to pass flexure or fatigue tests. The precautions already taken according to the regulations are thorough and extensive, and it is doubtless sound policy to go slowly in making hard and fast stipulations as to further mechanical tests.

In conclusion, the writer would again express his appreciation of Mr. Vaughan's paper, and the hope that something tangible and definite along the lines suggested by the author will, after the papers and many discussions on the subject during the last fourteen or fifteen years, be embodied in the Mining Regulations.

Mr. Laschinger further said he was very much interested in Mr. Cowell's contribution and the report from an expert at Home which he had just read.

With regard to certain tests made on wire ropes of incline shafts and from the microphotographs shown, which illustrated the hardening effect on the abraded outside skin of the worn wires, a very good lesson could be drawn. Mr. Vaughan had drawn attention at the last meeting to the fact that the outside skin of worn wires was exceedingly

brittle. If the outside of the worn hardened surface were filed off the behaviour of the wire under flexure tests would be different from what it would have been if the surface had not been filed off. It was quite possible that the hardening of the surface —the crown wires, as they were called in the report—was due to a very large extent to the fact that when those wires struck the rollers they attained a very high temperature, and it was not so much the pressure that caused the alteration in the structure of the steel as the fact that the temperature of the crown, for a very small depth, had been so highly raised that the microscopic structure, the physical quality of the steel, had been altered. He had already expressed the view that, owing to kinetic shocks, wire ropes in incline shafts were not as safe as they were in vertical shafts with the same factor of safety and the same length of rope, and any other factor which would aggravate those adverse conditions of incline as against vertical winding should be noted and due allowance made when ordering ropes for incline shafts.

J. R. Cowell observed that Mr. Stead had said that, according to the photographs, there were no marks which would indicate heating and cooling. Mr. Stead had put stress on that point, showing that the result was due to friction and pressure rather than heat.

THE FACTOR OF SAFETY OF WIRE ROPES USED FOR WINDING IN MINE SHAFTS.

Paper by J. A. Vaughan (Past President).

Author's Reply to Discussion.

June, 1918.

In starting to reply to the discussion on the paper, "The Factor of Safety of Wire Ropes Used for Winding in Mine Shafts," the author would like to reiterate that the proposals contained in this paper were not intended to alter the conditions under which winding is carried out at present on the Rand in shafts of moderate depth, say, less than 2,500 feet. This appeared to be quite clear from the statement on pages 98 and 99 of the November, 1917, *Journal*, viz.:—

"Seeing that a minimum factor of safety 6 has been found sufficient at shallow and moderate depths, and seeing that the rope denoted by $y_1=8$ provides for a factor of safety of 6 within the danger zone, *i.e.*, the area to the left of the critical curve in the Diagram 2, the writer proposes to allow this rope, or a rope with the same capacity-factor, to go down further, to depths where the factor of safety is 5 or even 4·5."

From Diagram 2, it can be seen that, for a rope whose $y_1=8$, of 120 tons steel, at 3,000 feet the factor of safety (y_2) will be 5·54, and at 4,000 feet it will be 5·0.

Mr. Roger Price missed this point and imagined that the author insisted on the values $y_1=8$ and $y_2=4·5$ being simultaneously applied to a winding rope, for any depth. Hence, in his *redutio ad absurdum*, he arrived at the value of 61·7 tons for "B," the available breaking stress of the steel wire. This rope would most probably not comply with Regulation 17 (2), which states:—

"Every rope used for winding purposes . . . . shall be made of steel wire, and the gauge of the wires used in the construction of such rope shall be suited to the diameter of the sheaves and drums fitted."

To prevent any misconception, however, the author proposes that, after the words "steel wire" in Regulation 17 (2), the qualifying clause be added—"of ultimate tensile strength of not less than 100 tons (of 2,000 lbs.) per square inch." This would lead at the low limit to values of y_2 (minimum), 5·07 at 3,000 feet and 5·9 at 2,000 feet. These figures supply an answer to Mr. Price's contention that the author's scheme did not afford relief in the case of shafts as shallow as 3,000 feet. It may be again pointed out that by adopting the limit of $y_1=8$, a gradual reduction of y_2 is provided for outside the critical area, culminating in the value $y_2=4·5$ at a depth of $\frac{1750}{36}$ B (5,250 feet in the case of B=108, *i.e.*, 90 per cent. of 120 tons per square inch), while there is a gradual increase of y_2 inside the critical area leading to the value $y_2=8$ at the surface.

Mr. Laschinger appealed against this proposed raising of y_2 above 6. In spite of his opinion to the contrary, it must be maintained that if his ideas were carried into effect, a "stepped" system would be the result, the value of y_1 from 8 at 2,250 feet to 7·7 at 2,000 feet, and 6·75 at 1,000 feet depth.

The author in the original paper, and Mr. E. G. Izod in his contribution to the discussion, pointed out from statistics that no hardship would be caused by fixing the minimum value of y_1 at 8 for all depths. There would be no interference with present practice in the shallower shafts, which means, of course, that engineers have not, up to the present, taken advantage of the apparently permissible reduction in y_2 to the minimum of 6, for reasons which will be shown later.

Thanks are due to Mr. A. C. Whittome for so completely disposing of Mr. Roger Price's criticisms dealing with the author's method of assessing the breaking load of a wire rope, as also its weight per foot. Nothing remains to be added with respect to these matters, except to point out that is exhibited, of 6/1 construction, in which all the strands are broken, but not completely, the soft core wire in each case remaining intact. Again, in a test when a single strand breaks in advance, there is, owing to the slack given out, a release of the stress on the remaining five strands, and one or more of these will break before the maximum test load is again reached.

Mr. A. E. du Pasquier's remarks showed a thorough understanding of the author's arguments and calculations, and it was pleasing to note that his considerable experience in winding problems permitted him to acquiesce in the proposal to reduce y_2 when outside the danger zone, and to state that this would be in sympathy with the modern tendency in the direction of winding heavier loads at slower speeds.

in the cases of the larger ropes, of diameters 1·5 in. to 2 in., although the test results of new ropes do disclose less than a 90 per cent. efficiency, this is caused by the nicking of the wires at their points of contact between adjacent strands, the number of point contacts to bear the test load being less in proportion than in the cases of smaller ropes. This apparent inefficiency should be lessened during the working life of the rope as its ultimate strength decreases. The nicking effect of round wires against each other is of small importance compared to that of other possible defects.

While alluding to test results of new ropes, it may be well to mention that Mr. A. C. Whittome was not quite correct in stating that it was unknown for four strands to break at once. This does occasionally occur, as also does the fracture of all six strands at once. An interesting specimen

The author cannot, however, agree with him when he states that the proper use to make of the reduction in y_2 is to employ steel of a lower ultimate tensile stress. There appears no reason to question the reliability of wires drawn up to as high as 140 short tons.

The successful experience of the Robinson Deep G.M. Co., with $1\frac{3}{8}$ in. diameter ropes of about 140 tons tensile, was fully set forth in certain communications in the *Journals* of this Institution of January and February, 1913.

Of 234 ropes recently tested, of diameters from 1 in. to 2 in., the wires of 33 were of grade 110/120 tons; 125 from 120/130 tons, and 76 from 130/140 tons.

In very few of these cases could the rule y_2 min. = 6 have been the determining factor, so that one is forced to the conclusion